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The American Historical Review appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in 914 Atwater, Indiana University, Bloomington, Indiana 47405.

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes over \$30,000, \$50.00 annually; \$20,000–\$29,999, \$42.00; \$15,000–\$19,999, \$35.00; \$10,000–\$14,999, \$25.00; below \$10,000 and joint memberships \$15.00; associate (non-historian) \$25.00; life \$1,000. The proportion of dues allocated to the *AHR* is \$17.00. Subscription rates effective for volume 87: Class I, *American Historical Review* only, United States, Canada, and Mexico \$43.00, foreign \$47.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1(a) and 2(a)—immediately preceding the advertisements.

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CHARLES INGRAO, currently associate professor of history at Purdue University, received his Ph.D. from Brown University in 1974. His *In Quest and Crisis: Emperor Joseph I and the Habsburg Monarchy* appeared in 1979, and in an expanded German edition this spring. He is currently working on the problem of enlightened absolutism in the German states, under the auspices of a fellowship from the Alexander von Humboldt Foundation. The article featured in this issue has been drawn from his projected book-length study of Hessian state and society under Landgrave Frederick II (1760–1785).

WALTER A. MCDUGALL, an assistant professor of history at the University of California, Berkeley, is completing a book on the political history of the origins and early years of space technology, begun while a Fellow of the Woodrow Wilson Center at the Smithsonian Institution. A native of Chicago, he was educated at Amherst College (1968) and, after service in Vietnam, the University of Chicago (1974), where he studied under William McNeill. His first book, *France's Rhineland Diplomacy, 1914–1924: The Last Bid for a Balance of Power in Europe*, was published by Princeton in 1978. A triad of concerns—the interplay of international rivalry and technological change, the legal problems of nonterritorial regions (oceans and Antarctica, for example), and a desire to expand the traditional purview of diplomatic history—triggered his current study of the roots and domestic impact of the space technological revolution.

BENJAMIN F. MARTIN is associate professor of history at West Virginia Wesleyan College. He studied with Lamar Cecil at the University of North Carolina, where he took his Ph.D. in 1974. He is the author of *Count Albert de Mun: Paladin of the Third Republic* (1978) and of several articles dealing with the political and social history of modern France. His book-length study of politics and trials during the Belle Époque is being considered for publication, and he has undertaken a long-term project on the criminal justice system of France in the nineteenth and twentieth centuries.

ALDEN T. VAUGHAN, professor of history at Columbia University, specializes in America's colonial period. He received his undergraduate degree from Amherst College and his graduate degrees from Columbia, where he studied principally with Richard B. Morris. Vaughan's major areas of research are early American race relations, especially Anglo-Indian contact, and American Puritanism. His publications include *New England Frontier: Puritans and Indians, 1620–1675* (1965, 1979), *American Genesis: Captain John Smith and the Founding of Virginia* (1975), articles in the *William and Mary Quarterly* and other journals, and several anthologies, most recently *Puritans among the Indians: Accounts of Captivity and Redemption, 1676–1724* (with E. W. Clark, 1981). He is currently writing a comprehensive analysis of Indian-European relations in British America.

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From White Man to Redskin: Changing Anglo-American Perceptions of the American Indian

ALDEN T. VAUGHAN

WHEN THOMAS JEFFERSON WROTE his *Notes on the State of Virginia* (1784), he professed to draw heavily “on what I have seen of man, white, red, and black.” Here, as elsewhere, Jefferson demonstrated a knack for phraseology: he was probably the first public spokesman to use the tricolor metaphor that has flourished, with minor rearrangement, ever since.¹ And now, with the historical profession’s increasing attention to race and ethnicity, “red, white, and black” is enjoying a rhetorical boom. Attracted by the color trilogy’s symmetry and symbolism, scholars have recently employed it in numerous book and article titles. Color terminology is as firmly fixed in writings on early America as it is in conversations about modern race relations.²

The tacit justification for using the chromatic metaphor in colonial and early national studies—apart from its ironic parody of “red, white, and blue”—is basically sound: Indians, Europeans, and Africans played central roles in early America, and the commonly accepted color labels are convenient and unambiguous, even though they exaggerate human complexions. Implicitly, if not explicitly, such usage further

A preliminary version of this essay was presented to an American history seminar in January 1978 at the University of Maryland, where I profited from comments by several participants. I am also indebted to James Axtell, Francis Bremer, Virginia Carr, Daniel Richter, and Bernard Sheehan for perceptive critiques and to the staffs of the Folger Shakespeare Library and the American Antiquarian Society for frequent, skillful, and genial assistance.

¹ Jefferson, *Notes on the State of Virginia* (1784), ed. William Peden (Chapel Hill, N.C., 1955), 59. More than a decade earlier, at least one writer used all three colors in reference to America’s inhabitants but not in a single phrase; Bernard Romans, *A Concise Natural History of East and West Florida* (New York, 1775), 37–38. Still earlier, Benjamin Franklin had used similar terms in a single essay but with a different meaning for red; Franklin, “Observations concerning the Increase of Mankind, Peopling of Countries, etc.,” in Leonard W. Labaree *et al.*, eds., *The Papers of Benjamin Franklin*, 3 (New Haven, 1961): 234.

² Works that use one or more racial colors in their titles and deal, at least in part, with the colonial period include Gary B. Nash, *Red, White, and Black: The Peoples of Early America* (1974; 2d edn., Englewood Cliffs, N.J., 1982), and “Red, White, and Black: The Origins of Racism in Early America,” in Nash and Richard Weiss, eds., *The Great Fear: Race in the Mind of America* (New York, 1970); Wesley Frank Craven, *White, Red, and Black: The Seventeenth-Century Virginian* (Charlottesville, Va., 1971); Neal E. Salisbury, “Red Puritans: The ‘Praying Indians’ of Massachusetts Bay and John Eliot,” *William and Mary Quarterly* [hereafter, *WMQ*], 3d ser., 31 (1974): 27–54; James P. Ronda, “Red and White at the Bench: Indians and the Law in Plymouth Colony, 1620–1691,” *Essex Institute Historical Collections*, 110 (1974): 200–15; Wilcomb E. Washburn, *Red Man’s Land / White Man’s Law: A Study of the Past and Present Status of the American Indian* (New York, 1971); Norman J. Heard, *White into Red: A Study of the Assimilation of White Persons Captured by Indians* (Metuchen, N.J., 1973); Dwight W. Hoover, *The Red and the Black* (Chicago, 1976); and Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro, 1550–1812* (Chapel Hill, N. C., 1968). In a few instances, and certainly for Jordan’s work, the title is entirely justified by the historical context. But the use of “red” either in titles or in texts is, I contend, inappropriate in works concerning Indian-European relations in colonial America. (*Mea culpa*, too, in my earlier writings.) In addition to the many authors who refer anachronistically to “reds” and “red men” in writing about the colonial period, most other authors at least call the Europeans “whites,” thus implying a fundamental color difference.

suggests that a tripartite racial division, based to an appreciable extent on perceived differences in pigmentation, underlay colonial America's social and political policies. But such an interpretation and the assumptions on which it rests are misleading: not until the middle of the eighteenth century did most Anglo-Americans view Indians as significantly different in color from themselves, and not until the nineteenth century did red become the universally accepted color label for American Indians.³ To read later perceptions of Indian pigmentation into the first centuries of racial contact is fallacious, because, in general, it distorts the nature of early ethnic relations and, in particular, it obscures the evolution of Anglo-American attitudes toward the Indians.⁴

Color, of course, was not the only characteristic to shape British-American perceptions of non-Europeans. Also important in the formulation of attitudes toward the natives of Africa and America were cultural traits: religion, government, economy, language, technology, and social mores. Important too were physical traits other than skin color: stature, proportions, facial features, and hair texture. But from the dawn of England's intellectual awakening to the African and American continents in the mid-sixteenth century, color perceptions were fundamental to Anglo-American assessments of peoples strikingly different from themselves, because color was more than a matter of aesthetic preference. Pigmentation also symbolized a cluster of behavioral and biological characteristics. In the case of Africans—but not Indians—color prejudice combined with cultural and religious prejudice to place blacks in a tragically inferior status.⁵ That eventually happened to Indians, too, though not quite to the same extent and not until two centuries of culture contact had altered Anglo-American perceptions.

³ Europe's essentially dichromatic perception of human color—white and black, with minor intermediate shades—applied to Orientals as well: they, too, were initially seen as white. See, for example, Alessandro Valignano, S.J. (1580), as quoted in P. G. Rogers, *The First Englishman in Japan: The Story of Will Adams* (London, 1956), 17–18; and John Saris, *The Voyages of Captain John Saris*, Hakluyt Society Publications, 2d ser., 5 (London, 1900), 84. Also see Hiroshi Wagatsuma, "The Social Perception of Skin Color in Japan," in John Hope Franklin, ed., *Color and Race* (Boston, 1968).

In this essay I generally use "Anglo-Americans" rather than "European-Americans" (or "Euro-Americans") because English culture predominated in British America and subsequently in the early United States and because I have drawn primarily on English and American sources, although those sources often include translations from Continental tongues. I find no evidence that non-English colonists or their descendants had different perceptions or terminology; there seems to have been a European consensus on color perception. I cite Spanish, Dutch, and Swedish sources *passim*; for French Canadian sources, see note 24, below.

⁴ Winthrop Jordan, with whom I agree on almost every point and to whose work all students of early race relations are deeply indebted, erred, I believe, in arguing that colonial Americans saw Indians as lighter than they "really" were; *White over Black*, chap. 6. It is more likely that both modern historians and the general public—perhaps because of racial assumptions they cannot easily overcome—are determined to view the Indians as darker than they are. But the reality of color is more subjective than objective: how the Indians' color is judged in comparison to the Europeans' and Africans' was—and is now—largely a matter of social perceptions, which can change to meet the psychological needs of the observer. For useful insights into color and racism, see Franklin, *Color and Race*, especially the essays by Kenneth J. Gergen, Harold R. Isaacs, and C. Eric Lincoln. A serious misunderstanding of seventeenth-century color perception is G. E. Thomas's "Puritans, Indians, and the Concept of Race," *New England Quarterly*, 48 (1975): 3–27, which concludes, "No matter how hard the [seventeenth-century] Puritan tried to transform the Indian or how completely the Indian conformed, the cause was ultimately hopeless because the Indian could never become white." Although Thomas's article is unprofessional on several counts, it is occasionally and mistakenly cited as authoritative.

⁵ A recent wide-ranging analysis of slavery in the Western world concludes that "patterns of both race relations and prejudice are determined by power relationships" rather than by deep-seated racial bias. Such an interpretation, I believe, applies only to a minor extent in British America and seriously underestimates the importance of color bias among European-Americans. See William McKee Evans, "From the Land of Canaan to the Land of Guinea: The Strange Odyssey of the 'Sons of Ham,'" *AHR*, 85 (1980): 15–43.

To generalize baldly what this essay elaborates: English and American writers, and most likely the mass of their countrymen, believed at the outset of England's age of expansion that Africans were inherently and immutably black—a color fraught with pejorative implications—and that therefore Africans were fundamentally unassimilable even if they adopted English ways and beliefs. At the same time, Anglo-Americans believed that American Indians were approximately as light-skinned as Europeans—with all its implications—and thus would be assimilated into colonial society as soon as they succumbed to English social norms and Protestant theology. The first of these beliefs held fast throughout the colonial period and beyond; the second underwent a slow but drastic change in the late seventeenth century and throughout the eighteenth as Anglo-Americans shifted their perception of Indian color from innately white to innately dark and eventually to red. That transformation, reflecting a confluence of European and American ideas and events, signaled important changes in white America's attitudes toward the native population. The new attitudes, in turn, helped assure the Indians' continued segregation and heighten their exploitation in the nineteenth and twentieth centuries. Although the Indians' position in British America was always precarious, not until they were thought of as inherently inferior "redmen" rather than unenlightened "whites" did their separate and unequal status become firmly fixed in the American mind. Only then could the bulk of American writers hold that Indians were prevented by *nature*—rather than by education or environment—from full participation in America's democracy and prosperity.

EARLY ENGLISH CONTACT WITH AFRICA spawned a vast range of literature—plays, poems, sermons, and secular tracts, as well as accounts of travel and exploration—that reveals a profound antipathy toward African appearance and culture. As Winthrop D. Jordan's *White over Black* (1968) convincingly demonstrates, virtually all descriptions of the "dark continent" portray its inhabitants as unattractive, heathen, and grossly uncivil. In theory at least, the Africans' culture could be ameliorated; their physical characteristics could not. And, although several aspects of African appearance—stature, facial features, and hair texture, for example—displeased the English eye, most striking and disturbing was the darkness of African skin. Descriptions of African people invariably stress their blackness, always disapprovingly.⁶

Europeans accepted with little debate the prevalence and permanence of the African's dark complexion. Doubts focused instead on its cause—whether from tropical heat, disease, or divine judgment—and on the reasons for the degrees of blackness within the continent. An early example of an argument for environmental causation and its implications was André Thevet's *New Founde Worlde*, translated into English in 1568 and widely read in Britain. Extreme solar temperature, Thevet argued, drew heat from the heart and other interior parts, leaving Africans cold on

⁶ Jordan, *White over Black*, chap. 1; and Eldredh D. Jones, *The Elizabethan Image of Africa* (Charlottesville, Va., 1971). The first wave of English settlers used similar pejorative terminology in referring to Africans (as, of course, did later generations). On early colonial attitudes, also see Alden T. Vaughan, "Blacks in Virginia: A Note on the First Decade," *WMQ*, 3d ser., 29 (1972): 469–78.

the inside, scorched on the surface. His observations applied principally to the southern regions: "Those of *Arabia* and of *Egypt* are betweene black and white, others browne coloured whom we call white Moores, others are cleane black." Three decades later, George Abbot, master of University College, Oxford, and the future archbishop of Canterbury, summarized the prevailing English view of African color. By then, the causal explanation had largely shifted from geographical to theological, but the original color perception remained the same: "all the people in generall to the South, lying within the *Zona torrida*, are not only blackish like the Moores: but are exceedingly black . . . so at this day, they are named Negroes, as them, whome no men are blacker."⁷

As Abbot suggested, the English name for central and southern Africans came from their skin color. Throughout Europe, in fact, Africans were almost invariably labeled by color. To the English, they were "blacks," "blackamores," or "Negroes"; to the Spanish, Portuguese, and Italians, they were "negros" and "negras"; to the Dutch, they were "negers." And in each language the word for "black" carried a host of disparaging connotations. In Spanish, for example, *negro* also meant gloomy, dismal, unfit, and wretched; in French, *noir* also connoted foul, dirty, base, and wicked; in Dutch, certain compounds of *zwart* conveyed notions of anger, irascibility, and necromancy; and "black" had comparable pejorative implications in Elizabethan and Stuart England.⁸ An early seventeenth-century cleric neatly tied the contempt for blackness to the standard theological explanation of African complexion, which asserted that Africans were descended from Noah's disobedient son: "the accursed seed of *Cham* . . . had for a stamp [of] their fathers sinne, the colour of hell set upon their faces."⁹ Predictably, to the English eye, the lighter the skin, the less hellish its connotations. Poet Thomas Peyton incorporated the prevailing chromatic hierarchy into a verse description of Africa:

The Libian dusky in his parched skin,
The Moor all tawny both without and in,
The Southern man, a black deformed Elfe,
The Northern white like unto God himselfe.¹⁰

Thus, whether using an English word or borrowing an equivalent term from a Continental language, Englishmen in the sixteenth and seventeenth centuries subtly and succinctly reflected their contemptuous view of the African. Captain John Smith, who had seen far more varieties of mankind than had most of his contemporaries, articulated the assumed connection between color and character when he described Africa as "those fryed Regions of blacke brutish Negroes."¹¹

⁷ Thevet, *The New Founde Worlde* . . . , trans. T[homas] Hacket (London, 1568), ff. 25, 13v; and Abbot, *A Briefe Description of the Whole Worlde* (London, 1599), sig. C7. The belief that North Africans were considerably lighter than those on the rest of the continent is graphically demonstrated on an illustrated map reproduced from Willem Janszoon Blaeu, *Appendix Theatri A. Ortelii et Atlantis G. Mercatoris* . . . (Amsterdam, 1631), as "A New Description of Africa, 1631," appended to Jones, *Elizabethan Image of Africa*.

⁸ Emilio M. Martínez Amador, *English-Spanish and Spanish-English Dictionary* (Barcelona, 1945), 699; E. Roubard, ed., *A French and English Dictionary* (London, n.d.), 369; *Nieuw Engleisch Woordenboek*, pt. 2 (Zutphen, n.d.): 586; and Jordan, *White over Black*, 4–11.

⁹ [R. Wilkinson] *Lot's Wife: A Sermon Preached at Pauls Crosse* (London, 1607), 42.

¹⁰ Peyton, *The Glasse of Time, in the First Age* (London, 1620; reprint edn., New York, 1886), 142.

¹¹ Edward Arber and A. G. Bradley, eds., *Travels and Works of Captain John Smith, President of Virginia, and Admiral of New England, 1580–1631*, 2 vols. (Edinburgh, 1910), 1: 191, and 2: 705.

Not until the eighteenth century did Englishmen, either at home or in America, develop a somewhat comparable perception of Indians. Rather, Englishmen and Continental Europeans labeled the natives on the basis of culture, not color, and whatever pejorative connotations such terms carried were social rather than biological. No single label predominated, although since Columbian times "Indian," occasionally modified by "West" if the context did not distinguish American from Asian Indians, was the overwhelming favorite. "Natives" probably ranked second in frequency to "Indians." "Savages" (often spelled "salvages"), "barbarians," and "heathens" were also commonly used by Europeans, especially in the sixteenth century, as were, less frequently, "wild people" or "brutish people" and such neutral terms as "country people," "naturals," "inhabitants," "old inhabitants," or "old people."¹² "Americans" and "Virginians" denoted Indians until the swelling colonial population made such words ambiguous; henceforth, "Americans" usually meant European immigrants and their descendants, while the definition of "Virginians" shifted from the natives to the English settlers of the first permanent British colony. Similarly ambiguous, "natives" began as a term for Indians but was soon (though inconsistently) applied to the colonists.¹³

English and Continental writers rarely applied color terms to the original American inhabitants. Not that Europeans were unconcerned with the Indians' color. They were intrigued by it; for the two centuries after 1492, most published accounts of America gave at least some attention to Indian complexion, and many examined it meticulously. During the first decades after 1492, writers took pains to deny that the Indians were black, to counteract the prevailing supposition that all people in torrid latitudes ought to be as dark as Africans. (The discovery that the Indians were not black dealt a crippling blow to the geographical explanation of human color, although that argument in modified form lingered into the nineteenth century.) Columbus, for example, reported that the natives in the Caribbean Islands were "not at all black, but the colour of the Canarians, and nothing else could be expected, since this is in one line from . . . the Canaries."¹⁴ Voyagers to more southerly latitudes found that the inhabitants of the equatorial zone were not appreciably darker, which gave rise to more expansive descriptions of their color and to various causal explanations, especially after Europe realized that America was a separate land mass and its inhabitants were perhaps a distinct branch of mankind.

Most early chroniclers of America's natives called them olive, tawny, or brown, occasionally russet or yellow, and sometimes a combination of such colors. Sir Walter Raleigh, for example, joined two of the standard hues when he described the

¹² Although "Indians" was by far the most common appellation, a few writers eschewed it completely. Sir William Alexander, for example, used "savages," "natives," and "Americans," and Abbot simply referred to "the inhabitants," "the old people," "the people of . . .," and occasionally "the Americans." Alexander, *An Encouragement to Colonies* (London, 1624); and Abbot, *Briefe Description of the Whole Worlde*.

¹³ Other geographical terms, of course, were also used for Indians in specific localities or regions: Floridians, Mexicans, Brazilians, and the like. In the eighteenth century, "Americans" was frequently used by European biologists and occasionally by political writers to distinguish American natives from transplanted Europeans. "Virginians" was often applied to Indians in the early decades of the seventeenth century (during Pocahontas's visit to England in 1616–17, a contemporary referred to her as "the Virginian woman") but rarely thereafter.

¹⁴ Columbus, *The Journal of Christopher Columbus*, trans. Cecil Jane and L. A. Vigneras, Hakluyt Society Publications, extra ser., 38 (London, 1960), 24–25.

women of Guiana as “brown and tawnie.”¹⁵ English plays and poems of the era often resorted to more imaginative descriptions, reflecting both the variety of New World reports and the confusion they produced in the European mind. A masque of 1603 specified that *America* be represented by a woman in “a skin coate the colour of the iuyce of Mulberries,” and Thomas Peyton’s *The Glasse of Time* (1620) described “the American” as “Olive coloured of a sad French green.”¹⁶ Presumably, Peyton’s readers knew what he meant.

Surprisingly, in light of today’s wide acceptance of red as the American Indians’ color label, Europeans rarely associated that color with the Indians before the late eighteenth century, and the few such references are to red stains. Walter Bigges, who accompanied Sir Francis Drake to the West Indies in 1585–86, observed that the natives of Dominica had “their skin coloured with some painting of a reddish tawney.” George Percy corroborated Bigges’s judgment when the Virginia expedition of 1607 stopped at the island en route to the mainland: “their bodies are all painted red to keepe away the bitings of Muscetos.”¹⁷ A few years later, William Strachey described the coloring of Virginia’s Indians. “Their heades and shoulders they paynt oftenest,” he reported, “and those with red, with the roote of Pochone [pokeberry], brayed to poulder mixed with oyle of the walnut, or Beares grease.” In the mid-1630s Father Andrew White related that the Maryland Indians painted themselves “a darke read, especially about the head.”¹⁸ But neither “redskins” nor “reds” appears in the early literature, and the few references to “red Indians” denoted East Indians. “This World’s Sunne,” asserted John Davies in 1599, still clinging to the climatological explanation,

Makes the *More* black, & th’ European white,
Th’ *American* tawny, and th’ *East-Indian* red.¹⁹

¹⁵ Raleigh, *The Discoveries of the Large, Rich, and Bewtiful Empire of Guiana* (London, 1596), 95. For another description of the Indians of the North American coast as brown, see Peter Lindström, *Geographia Americana, with an Account of the Delaware Indians, Based on Surveys and Notes Made in 1654–1656*, trans. and ed. Amandus Johnson (Philadelphia, 1925), 191.

¹⁶ Thomas Champion, *Somerset’s Masque* (1613), as quoted in Allardyce Nicoll, *Stuart Masques and the Renaissance Stage* (London, 1938), 192; and Peyton, *The Glasse of Time*, 142. An early suggestion that American Indians differed in pigmentation from other peoples was offered by Samuel Purchas in 1613: “The tawney Moore, black Negro, duskie Libyan, Ash-coloured Indian, olive-coloured American, should with the whiter European become *one sheepe-fold*”; [Purchas] *Purchas His Pilgrimage* (London, 1613), 546. Although he retained this passage in subsequent editions, Purchas’s other writings indicate his acceptance of the prevailing belief that American natives were innately white.

¹⁷ Bigges, *A Summarie and True Discourse of Sir Francis Drake’s West Indian Voyage* (London, 1589; reprint edn., New York, 1969), 20; and [Percy] “Observations Gathered out of a Discourse of the Plantation of the Southerne Colonie in Virginia . . .,” in Samuel Purchas, comp., *Hakluytus Posthumus: or, Purchas His Pilgrimes*, 4 (London, 1625): 1686, and reprinted in Philip L. Barbour, ed., *The Jamestown Voyages under the First Charter, 1606–1609*, Hakluyt Society Publications, 2d ser., 136–37 (Cambridge, 1969), 1: 130.

¹⁸ Strachey, *The Historie of Travell into Virginia Britania* (1612), ed. Louis B. Wright and Virginia Freund, Hakluyt Society Publications, 2d ser., 103 (London, 1953), 71; and White, “A Briefe Relation of the Voyage unto Maryland,” in Clayton Colman Hall, ed., *Narratives of Early Maryland, 1633–1684*, Original Narratives of Early American History (New York, 1910), 42.

¹⁹ Davies, *Nosce Teipsum . . . Hymes of Astrae in Acrosticke Verse* (London, 1622), 40. The reason for the attribution of red skins to East Indians is obscure. Assuming that East Indians looked darker than American Indians but lighter than most Africans, the color descriptions make logical if not literal sense. Davies may have drawn his inspiration from a work of Philippe de Mornay, translated into English in 1581, which was less specific but employed a wider spectrum. The sun, according to Mornay, “maketh some folks whyte, some black, some read and some Tawny, . . . the Ethiopian blacke, and the Scotte yellowish.” Presumably, Mornay included the American Indians among the tawnies. Mornay, *A Woorkes concerning the Trewness of the Christian Religion*, trans. Sir P[hilip] Sidney and A. Golding (London, 1587), 21–22.

Part of the confusion over the Indian's color stemmed from the differences in complexion from region to region, from tribe to tribe, and even within tribes. David Ingram, a sailor on Sir Humphrey Gilbert's expedition to North America in 1568, reported that the Indians in the southern part were olive, those in the north tawny. In 1583 Antonio de Espejo observed that in the area that later became New Mexico "the women are whiter skinned than the Mexican women." A decade later, Thomas Blundeville insisted that Indians in hot zones are "browne bay like a Chestnut, and the higher they dwell to either of the Poles Arctique or Antarctique, the whiter most commonly they be."²⁰ And readers of Richard Hakluyt's *Divers Voyages Touching the Discoverie of America* (1582) could find in the accounts of coastal North America a considerable chromatic range. In Florida the Indians were "of tauny colour" and in future South Carolina they were "of colour russet, . . . not much unlike the Saracens"; Virginia's Indians were "more white than those that we founde before," while the Narragansetts of southern New England had "the colour of brasse, some of them encline more to whitnes: others are of yellowe colour."²¹

Clearly the Indians were not all of the same hue. Clearly too they were—on the surface at least—appreciably darker than Englishmen and other Europeans. But the reason for the Indians' color was debatable. In the sixteenth century, both heredity and environment had their champions; by the early seventeenth century, a consensus endorsed environment. Most chroniclers who knew the Indians well insisted that they were naturally white-skinned. That judgment had to overcome an earlier contention, which circulated widely in England, that Indians were dark-skinned: Richard Eden's *Decades of the New World* (1555), a compilation of travel narratives that extensively describes America's inhabitants, includes an account "Of the Colour of the Indians" by Francisco López de Gómara, a participant in the early Spanish conquest. He argued that West Indians were "in general eyther purple, or tawny lyke unto sodde quynses, or of the colour of chestnutes or olyves: which colour is to them natural and not by theyr goynge naked as many have thought." Gómara qualified his emphasis on heredity, however, by adding that "theyr nakednesse have sumwhat helped therunto" and by acknowledging wide variation within the Indies, "with suche diversitie as men are commonly whyte in Europe and blacke in Affricke, even with lyke variety are they tawny in these Indies, with divers degrees diversly inclynynge more or lesse to blacke or whyte." And a few pages later Gómara quoted, without comment, Jacobus Gastaldus's description of Newfoundland's inhabitants as "whyte people."²²

Perhaps in reaction to Gómara's insistence that some West Indians were naturally

²⁰ [Ingram] "The Relation of David Ingram," in David Beers Quinn, ed., *The Voyages and Colonizing Enterprises of Sir Humphrey Gilbert*, Hakluyt Society Publications, 2d ser., 84–85 (London, 1940), 2: 285; Espejo, *New Mexico; Otherwise the Voiage of Anthony of Espejo . . . in the Year 1583* (London, 1587), 191; and Blundeville, *M. Blundeville His Exercises, containing Sixe Treatises . . .* (London, 1594), f. 262v.

²¹ Hakluyt, *Divers Voyages Touching the Discoverie of America* (London, 1582; facsimile edn., Amsterdam, 1967), 86, 46, 50, 53. This edition has bracketed page numbers for the reader's convenience (the original carries only signature letters); an accompanying volume, David B. Quinn's *Richard Hakluyt, Editor*, has an index by Alison Quinn that gives modern locations for the various tribes described in Hakluyt's narratives.

²² Gómara, "Of the Colour of the Indians," in Edward Arber, ed., *The First Three English Books on America, (?1511–) 1555 A.D.* (Birmingham, 1885), 338, 345. Gómara's account of Indian color gained new prominence when it was reprinted and placed on the opening pages of Pietro Martire d'Anghiera *et al.*, *The History of Travayle in the West and East Indies . . .*, trans. Richard Eden and Richard Willes (London, 1577).

...the people of this tribe were
...from others when
they are in their general
...or at their
...feasts.



Figure 1: John White's portrait of an Indian near Roanoke, North Carolina, showing extensive body painting. Most of White's contemporaries attributed the Indians' coloration to such painting as well as to constant exposure to the sun. Reproduced from Paul Hulton and David Beers Quinn, *The American Drawings of John White*, 2 (London and Chapel Hill, 1964): 47, courtesy of the University of North Carolina Press.

dark, and to other observers' failure to account for the Indians' outward appearance, many subsequent authors stressed the innate whiteness of North American Indians. In 1578 George Best argued against climate as the cause of human complexion, partly because "under the Equinoctiall in *America* the people are not blacke, but white."²³ In 1587 René de Laudonnière, commander of an ill-fated Huguenot outpost in Florida, depicted the Indians there as olive. He added, however, that "when they are borne they be not so much of an olive colour and are far whiter. For the chief cause that maketh them to be this colour, procedes of annointings of oyle . . . and because of the sun which shineth hotte upon their bodies." A Dutch scholar, Jan Huygen van Linschoten, echoed Laudonnière. "When they come first into the world," he wrote in 1598, they "are not so blacke but very white: the blacke yellowish colour is made upon them by a certaine oyntment. . . . Their colour likewise changeth because they goe naked, and with the burning heate of the sunne."²⁴ Shortly after the turn of the century, Martin Pring explained that the Indians he had seen along the northeast coast were "a swart, tawnie, or Chestnut colour, not by nature but accidentally." William Strachey, writing of the Indians around the Chesapeake Bay, insisted in "A True Description of . . . Their Collour" that their tawny skin came partly from the sun and partly from a combination of "arsenickstone," ointments, and juices applied "so sone as they ar borne" and reinforced with daily painting. That Strachey may in some measure have been attempting to refute Gómara is suggested by the analogy both used to describe the resulting complexion. The Indians dye themselves, Strachey noted, because they consider it "the best beauty, to be neerest a kynd of Murrey, as a sodden Quince is."²⁵ Both "murrey" (reddish blue) and "quince" (dull yellow) appeared frequently in European descriptions of the Indians.

For English readers, the most convincing evidence of the Indians' whiteness undoubtedly came from their compatriots, such as Strachey, who settled temporarily or permanently in America. From Virginia, Captain John Smith described the Indians near Jamestown as "a colour browne when they are of any age, but they are borne white." Another participant in the Jamestown venture thought the Indians' skin to be tawny, "not so borne but with dying and paynting them selves, in which they delight greatly." And John Rolfe, whose marriage to Pocahontas made him an unimpeachable authority on the natural hue of at least one Virginia Indian,

²³ Best, *A True Discourse of the late Voyage of Discoverie for the Finding of a Passage to Cathaya* . . . (London, 1578), 28.

²⁴ Laudonnière, *A Notable Historie containing Foure Voyages Made by Certaine French Captayens unto Florida*, trans. R[ichard] H[akluyt] (London, 1587), 4; and Linschoten, *His Discours of Voyages into ye Easte & West Indies* (London, 1598), 220, 224. For French Canadian statements that the Indians were innately white, see W. L. Grant, ed., *Voyages of Samuel de Champlain, 1604–1618*, Original Narratives of Early American History (New York, 1907), 142; Marc Lescarbot, *History of New France*, trans. and ed. W. L. Grant, 3 vols. (Toronto, 1907–14), 3: 138–39; and Reuben Gold Thwaites, ed., *The Jesuit Relations and Allied Documents: Travels and Explorations of the Jesuit Missionaries in New France, 1610–1791*, 73 vols. (Cleveland, 1896–1901; reprint edn., New York, 1959), 1: 279, 2: 73, 38: 241, and 63: 265. Compare *ibid.*, 4: 205; and Chrestien Le Clercq, *New Relation of Gaspesia, with the Customs and Religion of the Gaspesian Indians*, trans. and ed. William F. Ganong, Champlain Society Publications, no. 5 (Toronto, 1910; reprint edn., New York, 1968), 241.

²⁵ Pring, "A Voyage Set out from the Citie of Bristoll . . . for the Discoverie of the North Part of Virginia, in the Yeere 1603 . . .," in Samuel Purchas, comp., *Hakluytus Posthumus: or, Purchas His Pilgrimes*, 20 vols. (Glasgow, 1905–07), 18: 325; and Strachey, *Historie of Travell into Virginia Britania*, 70.

attributed “their blackness” to ointments and smoky houses, which, he contended, had the same effect on Indian hides that smokehouses had on English bacon.²⁶

Virginia’s Indians differed in customs and language from those in many parts of British America, but not, apparently, in their innate color. In 1666 George Alsop described the Susquehannocks of Maryland and eastern Pennsylvania: “Their skins are naturally white, but altered from their originals by the several dyings of Roots and Barks, that . . . metamorphize their hydes into a dark Cinamon brown.” William Penn reported similarly on the natives of his colony. They are “of Complexion, Black,” he observed in 1683, “but by design, as the Gypsies in England.”²⁷ And several New England writers, whose familiarity with Indians could scarcely be doubted, reached the same conclusion. William Wood (1634), Thomas Morton (1637), and Roger Williams (1643), among others, testified that the Indians in their vicinity were naturally white.²⁸ Many of these early English colonists, including Penn, Williams, and missionary John Eliot, believed that Indians were probably descended from the lost tribes of Israel and that Indians therefore possessed essentially the same pigmentation as Europeans.²⁹

There were, of course, varying shades of pigment even among Europeans—from very light in Scandinavia to relatively dark in Spain—and occasionally Indians were compared to “swarthy Spaniards.” But several commentators insisted that the natives of North America were scarcely, if at all, darker than the average Englishman. “Their color is not much unlike the Sunne burnte Countrie man, who laboureth daily in the Sunne for his living,” according to Dionyse Settle, who had accompanied Martin Frobisher to the northern coast in 1577. “Their infants,” insisted Thomas Morton after several years among the Massachusetts Indians, “are of complexion white as our nation, but their mothers in their infancy make a bath of . . . such things as will staine their skinne for ever, wherein they dip and washe them to make them tawny.” And the Reverend William Crashaw, a staunch supporter of the Virginia enterprise, told of “a Virginian [Indian], that was with us here in *England*, whose skinne (though hee had gone naked all his life, till our men persuaded him to bee clothed) . . . was little more blacke or tawnie, than one of ours

²⁶ Arber and Bradley, *Travels and Works of Captain John Smith*, 1: 65; [Gabriel Archer (?)] “Description of the People,” in Barbour, *Jamestown Voyages under the First Charter*, 1: 103; and Samuel Purchas, “Of Virginia,” in his *Purchas His Pilgrimage* (1613; London, 1626), 844.

²⁷ Alsop, *A Character of the Province of Maryland* (London, 1666), in Hall, *Narratives of Early Maryland*, 366; and Penn to the Committee of the Free Society of Traders of the Province, residing in London [August 16, 1683], in Albert Cook Myers, ed., *Narratives of Early Pennsylvania, West New Jersey, and Delaware, 1630–1707*, Original Narratives of Early American History (New York, 1912; reprint edn., 1959), 230. Penn also likened the Indians’ complexion to an Italian’s.

²⁸ Wood, *New Englands Prospect* (London, 1634), 54–55; Williams, *Key into the Language of America* (London, 1643), 52; and Morton, *New English Canaan* (London, 1637), 32. Also see [William Morrell] “Morrell’s Poem on New England,” *Massachusetts Historical Society Collections*, 1st ser., 1 (1792): 135; and Thomas Lechford, “Plain Dealing: or, Newes from New-England,” *ibid.*, 3d ser., 3 (1833): 103.

²⁹ For a general discussion of the Lost Tribes theory, see Don Cameron Allen, *The Legend of Noah: Renaissance Rationalism in Art, Science, and Letters* (Urbana, Ill., 1949), 125–28. For Anglo-American subscribers to the theory, see, for example, Henry Whitfield, *The Light Appearing More and More towards the Perfect Day*, in *Massachusetts Historical Society Collections*, 3d ser., 4 (1834): 72–74, 93–95, 127–28; Penn to the Free Society of Traders, 236–37; and Williams, *A Key into the Language of America*, ed. John J. Teunissen and Evelyn J. Hinz (Detroit, 1973), 85–86. For the European side of the search for Jewish parallels, see Cecil Roth, *A Life of Menasseh ben Israel—Rabbi, Printer, and Diplomat* (Philadelphia, 1934); and Ronald Sanders, *Lost Tribes and Promised Lands: The Origins of American Racism* (Boston, 1978), esp. 363–72.

would be if he should goe naked in the South of *England*.”³⁰ Englishmen, then, perceived Indians as essentially white and, at most, a shade darker than themselves.

Despite this belief, and despite almost universal praise of the Indians' physique and physiognomy, Englishmen in Europe or America harbored a deep prejudice against almost all aspects of Indian culture. William Strachey is representative. Writing from Virginia in 1612, he described the Indians as “generally tall of stature, and streight, of comely proportion, and the women have handsome lymbes, slender armes, and pretty handes.” Of Indian beliefs and customs, however, Strachey was mostly contemptuous: “their chief god . . . is no other indeed then the devill”; “they are a people most voluptuous”; “vindictive and jelous they be” and “very barbarous.”³¹ In fact, many of the same pejoratives that had been applied to Africans appear in descriptions of Indians. In 1559 William Cuninghame described Africans as “blacke, Savage, Monstrous, & rude.” He considered Indians not much better—in some respects “comparable to brute beasts”—but the absence of a color label for the Indians points to a crucial difference in perception.³² For, although Englishmen's descriptions of Indians often are almost as negative as their accounts of Africans, their writings focus on customs, not bodies, on nurture, not nature. To be sure, the catalogue of imagined Indian shortcomings is long: nakedness, cannibalism, barbarism, idolatry, devil worship, brutality, lechery, indolence, and slovenliness. Some observers mitigated the Indians' alleged vices by reciting their virtues: hospitality, integrity, eloquence, hardiness, stoicism. But the lists of negative qualities, especially those compiled by armchair explorers, are usually more extensive and more emphatic. George Best, for example, had seen Indians in Newfoundland and admired their intelligence, modesty, and strength; he also thought them “brutishe and uncivil people” who “live in Caves of the Earth, and hunte for their dinners . . . even as the Beare, or other wilde beastes do. They eat rawe fleshe and fishe, and refuse no meate, howsoever it be stincking. They are desparate in their fighte, sullen of nature, and ravenous in their manner of feeding.” *The Historie of Travayle in the West and East Indies* (1577), the last of Richard Eden's influential translations of Continental narratives, offers a diatribe against the Mexican Indians that concludes, “God never created so corrupte a people for vice

³⁰ Settle, *A True Reporte of the Laste Voyage into the West and Northwest Regions, etc.*, 1577 (London, 1577), sig. c8; Morton, *New English Canaan*, 32; and Crashaw, *A Sermon Preached in London before the Right Honourable Lord La Warre* . . . (London, 1610), sig. e2. Crashaw thought West Indians considerably darker than Virginian Indians. An early eighteenth-century observer declared that Indians along the mid-Atlantic coast were born as white as Spaniards and Portuguese; Ann Maury, *Memoirs of a Huguenot Family* (New York, 1853), 350.

³¹ Strachey, *Historie of Travell into Virginia Britania*, 71, 88, 112, 104, 77. Strachey was less impressed than most of this contemporaries by Indian physiognomy, but he was quick to add that it was “nothing so unsightly as the Moores”; *ibid.*, 71. More representative of seventeenth-century European judgment was a French description of the Indians in Canada as “brown, tawny, and swarthy. . . . Their colour, however, does not lessen at all the natural beauty of the features of their faces.” Le Clercq, *New Relation of Gaspesia*, 241. A distinction should be drawn between armchair explorers in Europe, who had little if any contact with Indians, and actual explorers, who closely observed the Indians. The latter's descriptions are generally more favorable than are the former's. Both, however, usually praised Indian physique and physiognomy. For recent studies of early English writings on North American Indians, see H. C. Porter, *The Inconstant Savage: England and the North American Indian, 1500–1660* (London, 1979); and Karen Ordahl Kupperman, *Settling with the Indians: The Meeting of English and Indian Cultures in America, 1580–1640* (Totowa, N.J., 1980).

³² Cuninghame, *The Cosmographical Glasse, Conteynyng the Principles of Cosmographie, Geographie, Hydrographie, or Navigation* (London, 1559), 185, 201.

and beastliness, without any mixture of goodnesse and civilitie.” And George Abbot accused the Indians of being “naked, uncivill, some of them devourers of mans flesh, ignorant of shipping, without all kinde of learning, having no remembrance of history or writing among them, . . . utterly ignorant of scripture, or Christ, or Moyses, or any God.”³³ Even John Rolfe, requesting permission to marry Pocahontas, complained that her “education hath bin rude, her manners barbarous, her generation accursed, and . . . [she is] discrepant in all nurriture from my selfe.” Finally, King James I, in a statement that effectively summarized his subjects’ ethnocentric perception of America’s natives, called them “beastly *Indians*, slaves to the Spaniards, refuse of the world, and as yet aliens to the holy covenant of God.”³⁴



Figure 2: Seal of the Massachusetts Bay Colony (1675–90). Early Anglo-American expectations of Indian conversion are reflected in this seal, carved by John Foster, in which the Indian invites the English to “Come over and help us.” Reproduced courtesy of the American Antiquarian Society, Worcester, Mass.

James’s “as yet” was important. However much he and his countrymen abhorred Indian customs and beliefs, they believed that the Indians would one day be converted into neo-Englishmen. The English themselves, many writers readily admitted, had once been barbarian and pagan; their Roman conquerors had brought the twin blessings of civility and Christianity, for which Rome deserved

³³ Best, *A True Discourse of the Late Voyage of Discoverie*, 3d pagination: 51, 62; d’Anghiera *et al.*, *History of Travayle in the West and East Indies*, f. 458v; and Abbot, *A Briefe Description of the Whole Worlde*, sig. e2.

³⁴ Rolfe to Thomas Dale, 1613, in Ralph Hamor, *A True Discourse of the Present State of Virginia* (London, 1615), 64; and King James, *A Counter-Blaste to Tobacco* (London, 1604), sig. b2.

abundant praise.³⁵ Would not the Indians similarly respond? Once they saw the clear superiority of English life and faith, the American natives would embrace such advantages avidly. Early English reports from the New World encouraged such expectations. George Best recalled that Indians captured by the Frobisher expedition quickly “beganne to growe more civill”; George Peckham announced optimistically that the Indians were “thirsting after christianitie”; and the elder Richard Hakluyt drew on explorers’ chronicles to predict that the Indians in the area of prospective English settlement were ready “to submytte them selves to good government, and ready to imbrace the christian faythe.” Accordingly, much of the promotional literature of the late sixteenth and early seventeenth centuries made conversion and the uplifting of the Indians a major goal of English colonization.³⁶

In short, early English writings reflect a deep bias against Indian culture but not against Indian color, shape, or features; the American native was socially deplorable but physically admirable. The challenge to English colonists was therefore educational: the natives must be converted to Protestant Christianity, taught English language and law, and trained in the social mores of Tudor-Stuart England. They must become Englishmen in everything except geographic origin. Africans, however, largely for reasons of outward appearance, were already deemed permanently debased and actual or potential slaves. The difference in English attitudes toward Indians and Africans is underscored in Nathaniel Field’s epitome of the two major parts of the world that had recently been opened to English scrutiny: “wilde *Virginia*, Black Affricke.”³⁷ Black men would remain forever black and barbarous. England’s imperialists believed, but wildmen could be transformed by civilization and Christ.

IN 1634 WILLIAM WOOD, AN EARLY MASSACHUSETTS SETTLER who had recently returned to England, published a detailed description of New England’s flora, fauna, and native inhabitants. Wood left no doubt about the natural color of the Indians in that region. “Their swarthinness is the sun’s livery,” he insisted, “for they are born fair.” The first American edition of Wood’s book appeared in Boston in 1764. Its editor, Nathaniel Rogers, added a footnote to that observation: “this was one of the popular errors given into by our author.”³⁸

Rogers’s editorial comment reflects a fundamental shift in color perception from the early seventeenth to the late eighteenth century: by the later date, most Anglo-Americans no longer saw the Indian as inherently white. “The number of purely

³⁵ For examples of comparisons between Indians and ancient Britons, see Thomas Hariot, *A Briefe and True Reporte of the New Found Land of Virginia* (Frankfort, 1590), sig. G1; and Crashaw, *A Sermon Preached in London*, sig. C4v. Also see the paintings and discussions of ancient Picts and Britons in Paul H. Hulton and David B. Quinn, eds., *The American Drawings of John White*, 2 vols. (London, 1964).

³⁶ Best, *A True Discourse of the Late Voyage of Discoverie*, 63; Peckham, *A True Reporte . . . of the Newfound Landes . . .* (London, 1583), sig. B4; E. G. R. Taylor, ed., *The Original Writings and Correspondence of the Two Richard Hakluyts*, Hakluyt Society Publications, 2d ser., 76–77 (London, 1935), 2: 211, 339; and Crashaw, *A Sermon Preached in London*, sig. C3.

³⁷ Field, *A Woman Is a Weather-Cock* (London, 1612), sig. D3.

³⁸ Wood, *New Englands Prospect* (1634 edn.), 54–55, and *New England’s Prospect*, ed. Rogers (Boston, 1764), 94n.

white People in the World is proportionably very small," Benjamin Franklin observed in 1751. "All Africa is black or tawny. Asia is chiefly tawny. America (exclusive of the new Comers) wholly so."³⁹ Many of Franklin's contemporaries concurred in his basic judgment, although they did not necessarily accept his choice of colors. Jonathan Carver, who traveled through the American colonies in the 1760s, noted that "their skin is of a reddish or copper colour." And, although Carver denied that he would "enter into a particular enquiry whether the Indians are indebted to nature, art, or the temperature of the climate for the colour of their skin," he gave his opinion anyway. "It appears to me to be the tincture they received originally from the hands of their Creator; but . . . at what time the European whiteness, the jetty hue of the African, or the copper cast of the American were given them . . . I will not pretend to determine."⁴⁰ Robert Hunter, writing in the 1780s, similarly described the Indians' hue when he observed a boy "not contented with his natural copper color . . . [who] was painted red in different places." A more explicit statement of the Indians' innate darkness and their efforts to be even darker appeared in John Filson's *Kentucke* (1774): "the Indians are not born white; and take a great deal of pains to darken their complexion, by anointing themselves with grease, and lying in the sun. They also paint their faces, breasts and shoulders, of various colours, but generally red."⁴¹

The perceptual shift from Indians as white men to Indians as tawnies or redskins was neither sudden nor universally accepted. Throughout the eighteenth century, some authors insisted that the Indians were inherently white. After a brief residence in Pennsylvania in the early 1750s, German immigrant Gottlieb Mittelberger reported—perhaps from personal observation, perhaps from reading William Penn—that the Indians "are born as white as we ourselves are."⁴² More impressive dissent from the new consensus came from James Adair, a long-time supervisor of Indian affairs and an authority on Indian life. The first page of his *History of the American Indians* (1775) stated unequivocally that the "copper or red-clay colour" of the American Indians was "merely accidental, or artificial"; following most sixteenth- and seventeenth-century writers, he ascribed Indian complexion to climate and ointments. Yet at times even Adair hinted at a fundamental color distinction between Europeans and Indians. "All the Indians," he noted, "are so strongly attached to, and prejudiced in favour of, their own colour, that they think as meanly of the whites, as we possibly can doe of them."⁴³ Adair thus had it both ways: he considered the Indians innately white—which was necessary to his belief in their Jewish origin—yet he implicitly reinforced the image of dark-skinned natives and white-skinned Europeans viewing each other as distinct color categories.

That perception had been growing since the late seventeenth century. At first it

³⁹ Franklin, "Observation concerning the Increase of Mankind," 234.

⁴⁰ Carver, *Travels through the Interior Parts of North America in the Years 1766, 1767, and 1768* (London, 1778), 223–24.

⁴¹ [Hunter] *Quebec to Carolina in 1785–1786: Being the Travel Diary and Observations of Robert Hunter, Jr., . . .*, ed. Louis B. Wright and Marion Tinling (San Marino, Calif., 1943), 36; and Filson, *The Discovery, Settlement, and Present State of Kentucke* (Wilmington, Del., 1784), 98–99.

⁴² Mittelberger, *Journey to Pennsylvania*, ed. Oscar Handlin and John Clive (Cambridge, Mass., 1960), 63. Mittelberger described the Indians' acquired color as "black-yellow."

⁴³ Adair, *The History of the American Indians . . .* (London, 1775; reprint edn., New York, 1968), 1–4.

appeared as a contention that stains and paints increased, rather than caused, the Indians' darkness. "They are of a tawny colour," reported a Virginia colonist in 1689, "and they make themselves more so, by annointing their bodies with bear's grease." Soon after the turn of the century, former South Carolina governor John Archdale attested that the "Natives are somewhat Tawney, occasioned, in a great measure, by Oyling their Skins, and by the naked Raies of the Sun." Similarly, John Lawson in 1709 described the Indians of the Carolinas as "tawny, which would not be so dark, did they not dawb themselves with Bears Oil, and a colour like burnt Cork."⁴⁴ By 1728, when William Byrd published his *History of the Dividing Line*, the Indians' color seemed indisputably darker than European-Americans', but Byrd thought it could be "blanched" by two generations of intermarriage. Other commentators, by mentioning the Indians' dark hue without attributing it to environmental or cultural influences, implied a natural darkness. "Their skins," according to Daniel Gookin, supervisor of Indians in Massachusetts, "are of a tawny colour, not unlike the tawny Moors in Africa." Reverend Samuel Lee, also of Massachusetts, described the Indians as "tawny colourd: like the Tartarians."⁴⁵ Writing in the last quarter of the seventeenth century, neither Gookin nor Lee attributed the dusky hue to stains or the sun, as had most of their New England forebears.

The shift in color ascription, explicit or implicit, is not the only sign that European perceptions had changed. The terminology of ethnic identification also altered significantly between the early years of English colonization and the eve of the American Revolution. Almost without exception, sixteenth- and seventeenth-century writers referred to people of the Old World as Englishmen, Spaniards, Frenchmen, and so forth, often as Christians, seldom as whites except to distinguish them from blacks. The few exceptions suggest that the contrast between the Indians' *acquired* color and the Europeans' *natural* hue provided some observers with a handy label for Europeans of whatever nationality, but such usage remained rare in Indian-European contexts until the eighteenth century. A New Netherland document of 1652, for example, complained that "some malicious and evil disposed persons have not scrupled to inform . . . the Indians what sum and price the Dutch or Whites are giving each other for small Lots," but few if any other seventeenth-century Dutch documents use "whites" (*blancken*) in Indian contexts. The author of the anomalous statement from 1652 may have previously served on the African coast, as had many early Dutch officials, and hence have drawn on the vocabulary of black-white relations in the Dutch slave trade.⁴⁶

⁴⁴ [John Clayton (?)] "An Account of the Indians in Virginia," ed. Stanley Pargellis, *WMQ*, 3d ser., 16 (1959): 230; Archdale, *A New Description of . . . Carolina* (London, 1707), 7; and Lawson, *A New Voyage to Carolina* (London, 1709), 171. Also see John Brickell, *The Natural History of North Carolina* (Dublin, 1737), 294–95. Brickell's volume is almost entirely plagiarized from Lawson's *New Voyage*.

⁴⁵ [Byrd] *The Prose Works of William Byrd of Westover: Narratives of a Colonial Virginian*, ed. Louis B. Wright (Cambridge, Mass., 1966), 160–61, 221–22; Gookin, *Historical Collections of the Indians in New England* (Boston, 1792; facsimile reprint edn., New York, 1972), 4; and Lee to Nehemiah Grew, 1690, in "Letters of Samuel Lee and Samuel Sewall Relating to New England and the Indians," *Transactions of the Colonial Society of Massachusetts*, 14 (1911–13): 147. Also see Carl Bridenbaugh, ed., "Patrick M'Roberts' Tour through Part of the North Province of America," *Pennsylvania Magazine of History and Biography*, 59 (1935): 174.

⁴⁶ E. B. O'Callaghan, ed., *Laws and Ordinances of New Netherland, 1638–1674* (Albany, N.Y., 1868), 131. The original Dutch manuscript is in the New York State Archives, Albany. The rarity of Dutch use of *blanck* in Indian contexts is attested by Charles Gehring of the New Netherland Project; personal communication, November 2, 1981. On Dutch officials who went from Africa to America, see Jordan, *White over Black*, 172–73.

The first seventeenth-century English usage of “whites” in an Indian context appears in a portion of George Fox’s journal of his American travels, recorded in the 1670s but not published until the twentieth century. Fox reported a New England Indian’s account of an ancient belief that “a white people should come in a great thing of the sea.” If the transcription from Fox’s notes is accurate, Indians perhaps adopted color terminology before Anglo-Americans did, although the Indian’s reference may have been to outward rather than innate color—a parallel to the early Europeans’ use of “tawny” and “sodden quince.”⁴⁷

No such ambiguity clouds a South Carolina law of 1696 “for . . . Determining all Causes and Controversies between *White Man* and *Indian*.” Among Carolina legislators at least, and presumably among some of their contemporaries, “whites” was emerging as a legitimate label for Americans of European descent in lieu of “Christian” and “English.” Perhaps the South’s recent influx of Africans prompted an increased sensitivity to pigmentation and encouraged color classifications: the earliest surviving instances of such terminology appear in Southern documents. But authors and officials in Northern colonies were not far behind. Cotton Mather’s *Negro Christianized* (1706) used “whites” primarily in contrast to blacks, but in one passage he differentiated both from a third division of mankind: “As if the great God went by the *Complexion* of men, in His Favours to them! As if none but *Whites* might hope to be Favoured and Accepted with God! Whereas it is well known that *Whites*, are the least part of Mankind. The biggest part of Mankind, perhaps, are Copper-Coloured; a sort of *Tawnies*.” By the 1730s “whites” appeared frequently in Pennsylvania Council minutes and in Indian treaties.⁴⁸ By the second half of the eighteenth century, “white” had become a common appellation for a European of whatever nationality or whatever longevity in America. (National labels, of course, continued to be used where appropriate, especially to distinguish among several groups of European colonists.) For example, Philadelphia naturalist John Bartram usually referred to “Indian” and “English” in his published *Travels* (1751), but he occasionally used “whites” and “white men” to mean Europeans in general. Similarly, Jefferson asserted in his *Notes on the State of Virginia* that he considered

⁴⁷ [Fox] *Journal of George Fox*, ed. John L. Nickalls (Cambridge, 1952), 624. Nearly a century earlier, Arthur Barlowe of the Roanoke outpost described the Indians as “yellowish” and Europeans as “white people,” and he reported that the Indians were amazed “at the whiteness of our skinnies.” Barlowe was writing before extensive contact had altered Anglo-American notions of Indian pigmentation; the Indians, however, must have been puzzled by people who clothed their bodies and used no dyes or ointments. Also at Roanoke, Ralph Lane used “white men” to mean Europeans of unspecified nationality. “Arthur Barlowe’s Discourse of the First Voyage,” in David Beers Quinn, ed., *The Roanoke Voyages, 1584–1590*, Hakluyt Society Publications, 2d ser., 104–05 (London, 1955), 1: 102, 111–12; “Ralph Lane’s Discourse on the First Colony,” *ibid.*, 261.

⁴⁸ John D. Cushing, ed., *The Earliest Printed Laws of South Carolina, 1692–1734*, 2 vols. (Wilmington, Del., 1978), 1: 118, *The Earliest Printed Laws of New Jersey, 1703–1722* (Wilmington, Del., 1978), 22, *The Laws of the Province of Maryland* (Wilmington, Del., 1978), 199, and *The Earliest Printed Laws of the Province of Georgia, 1755–1770*, 2 vols. (Wilmington, Del., 1978), 1: 15; Samuel Hazard, ed., *Minutes of the Provincial Council of Pennsylvania . . .*, 16 vols. (Philadelphia, 1838–53), 4: 281; Mather, *The Negro Christianized* (Boston, 1706), 24; Donald H. Kent, ed., *Early American Indian Documents: Treaties and Laws, 1607–1789*, volume 1: *Pennsylvania and Delaware Treaties, 1629–1737* (Washington, 1979), 291, 375, 404, 428; and Lawson, *New Voyage to Carolina*, 233. The carry-over of color terminology from black-white to Indian-European contexts is illustrated by the writings of an early eighteenth-century Carolinian who often used “whites” in contrast to Africans and occasionally in contrast to Indians. In a few instances, he referred to “white, black, & Indians.” [Francis Le Jau] *The Carolina Chronicle of Dr. Francis Le Jau, 1706–1717*, ed. Frank J. Klingberg (Berkeley and Los Angeles, 1956), 22, 39, 155.

"the Indian . . . to be in body and mind equal to the white man," and he clung to such terminology whenever he compared natives and European-Americans.⁴⁹

Apparently, Indians adopted a similar vocabulary—if we can accept the surviving translations—as George Fox's journal entry implied and as several eighteenth-century sources corroborate. As early as 1687, an Onandaga sachem contended that the kings of England and France "are both of one Skinn meaning [the interpreter added] white Skinned, & not brown as they [*sic*] Indians are." This evidence of an emergent Indian sense of color categories and the implication that the differences were innate became more frequent and more explicit in the eighteenth century. In 1751 John Bartram quoted an Indian who advocated "peace and good harmony between the *Indians* and *White People*"; a quarter-century later missionary Samuel Kirkland recorded an Indian spokesman as saying, "We have no . . . inclination of interfering in the dispute between Old England and Boston; the white people may settle their own quarrels between themselves." Indian speeches even applied such words in retrospective accounts, as in an oration of 1773, which began with a brief resumé of Indian-European contact, "Brother, when we first saw the White People. . . ."⁵⁰

Anglo-American usage of *tawny* also reflected the new emphasis on color terminology. Until the last quarter of the seventeenth century, it was almost always an adjective, modifying Indians or natives; thereafter, it increasingly became a noun. In 1680, for example, Morgan Godwyn in *The Negro's and Indian's Advocate* discussed the conversion to Christianity of "Tawneys and Blacks"; in the next several decades many authors employed similar terminology.⁵¹ No doubt the temptation was strong among Anglo-Americans and Indians to find generic words to describe themselves and each other. Europeans had applied—or, rather, misapplied—"Indians" to the natives since 1492 but had no convenient term for themselves. "Europeans" was too broad for most contexts; "Christians" soon lost its diagnostic precision; and British America's growing ethnic diversity made "Englishmen" inappropriate. To some extent, perhaps, the coinage of color terms was

⁴⁹ Bartram, *Observations on the Inhabitants, Climate, Soil . . . in His Travels from Pennsylvania to Onondago* (London, 1751; reprint edn., Rochester, N.Y., 1895), 22, 57, 77; and Jefferson, *Notes on the State of Virginia*, 59–60, 185, 186. For other post-1750 uses of "whites" in Indian-European contexts, see, for example, Sylvester K. Stevens and Donald H. Kent, eds., *Wilderness Chronicles of Northwestern Pennsylvania* (Harrisburg, Pa., 1941), 236, 268, 287, 289. Indian uses of "whites" and "white men" proliferated in the late eighteenth and early nineteenth centuries, judging from the recorded speeches of the Delaware Prophet, Logan, Cornplanter, Handsome Lake, and others. For examples, see Anthony F. C. Wallace, *The Death and Rebirth of the Seneca* (New York, 1969), 120, 124, 133, 223, 224, 241, 265, 299.

⁵⁰ Lawrence W. Leder, ed., *The Livingston Indian Records, 1666–1723* (Harrisburg, Pa., 1956), 115; Bartram, *Observations on the Inhabitants*, 22; Peter Force, ed., *American Archives . . . : A Documentary History of . . . the North American Colonies*, 4th ser., 2 (1774): 842; and "Extract of the Minutes of a Congress with the Chiefs of the Six Nations," April 17, 1773, in Edmund B. O'Callaghan, ed., *Documentary History of the State of New York*, 4 vols. (Albany, 1849–51), 2: 580. During the eighteenth century, Indian myths about their origins and early history often included white-skinned deities or invaders; see, for example, Wallace, *Death and Rebirth of the Seneca*, esp. 242–48. Also see Wallace, ed., "Halliday Jackson's Journal to the Seneca Indians, 1798–1800," *Pennsylvania History*, 19 (1952): 117–47, 325–49.

⁵¹ Godwyn, *The Negro's & Indians Advocate* (London, 1680), 4. Cotton Mather was especially fond of using "tawny" as a noun rather than as an adjective, usually when describing Indian military activities. See, for example, his *Magnalia Christi Americana: or, The Ecclesiastical History of New-England . . .* (London, 1702), bk. 7, p. 87, *Good Fetch'd Out of Evill: A Collection of Memorables Relating to Our Captives* (Boston, 1706), 38, and *The Negro Christianized: An Essay to Excite and Assist That Good Work* (Boston, 1706), 24.

inevitable as blacks became more numerous and as Indians came increasingly to be seen in ethnic (and eventually racial) rather than national or tribal terms. But the persistence of "Indian" and the tardy appearance of "redmen" on the linguistic scene suggest a lingering reluctance to perceive the Indians in essentially racial terms, a reluctance that weakened, however, as the eighteenth century wore on.

Other, more subtle, indications of a growing pejorative perception of Indian color appear in American writings of the late seventeenth and early eighteenth centuries. Some are vague, as in Cotton Mather's tale of a "bewitched" girl's encounter with the devil: "hee was not of a *Negro*, but of a *Tawney*, or an *Indian Colour*." Even if Mather meant an Indian's *acquired* hue, the assignment of "an *Indian Colour*" to the devil tells something about Mather's—or the girl's—image of Indians.⁵² Also vague but at least as suggestive in its implications is Governor William Gooch's commentary on the Virginia law of 1724 for "the better government of Negroes, Mulattoes, and Indians," which deprived such men of the political franchise, even if they were free and owned property. In a series of letters to Alured Popple in 1735–36, Gooch explained that the basic issue was complexion: dark-skinned inhabitants were disfranchised purely on the basis of color. Gooch admitted that the number of free blacks and mulattoes "is so inconsiderable, that 'tis scarce worth while to take any Notice of them," but a dark skin, he insisted, should not disable anyone who had attained freemanship. He said nothing about Indians. Gooch either tacitly included them among blacks and mulattoes, or else none had been accorded freemanship. That he probably subsumed Indians under the rubric of mulattoes is strongly suggested by the Virginia Assembly's declaration of 1705 that "the child of an Indian and the child, grand child, or great grand child of a negro shall be deemed, accounted, held and taken to be a mulatto." In any event, the implication of Gooch's remarks and the intention of Virginia's legislation were to include Indians among the colony's colored population.⁵³

Similar intentions appear in other colonial statutes. During the seventeenth century, specific laws rarely applied to both Indians and blacks; the two groups were usually accorded separate and distinct legislative attention. Eighteenth-century lawmakers, however, often lumped them together. New York, for example, frequently proscribed the activities of its "Negro, Indian or Mulatto" populations; Southern colonies used similar language but often added a category of "mustizoes" (part Indian, part black or white). Laws against intermarriage are particularly telling. As early as 1691 Virginia passed an antimiscegenation law

⁵² Mather, "A Brand Pluck'd Out of the Burning," Mather Family Papers, American Antiquarian Society, box 6, folder 6, p. 3, printed with minor variations in George Lincoln Burr, ed., *Narratives of the Witchcraft Cases, 1648–1706*, Original Narratives of Early American History (New York, 1914), 261. If the "Indian color" designation came from Mercy Short rather than from Mather, it probably stemmed from her brief captivity by Indians in 1690—an experience that might well have encouraged her to associate Indians and devils. On such associations generally in the Puritan mind, see William S. Simmons, "Cultural Bias in New England Puritan Perceptions of Indians," *WMQ*, 3d ser., 38 (1981): 56–72.

⁵³ For Gooch's letters, see Emory G. Evans, ed., "A Question of Complexion: Documents concerning the Negro and the Franchise in Eighteenth-Century Virginia," *Virginia Magazine of History and Biography*, 71 (1963): 411–15. (My interpretation of Gooch's statements differs appreciably from that of Evans.) The Virginia statute of 1705 is in William Waller Hening, comp., *The Statutes at Large; Being a Collection of the Laws of Virginia, from the First Session . . .*, 3 (Richmond, 1815): 252.

aimed at Indians as well as blacks. (Its 1662 law, while using color as the basis for racial separation, did not mention Indians.) To be sure, Virginia's antimiscegenation laws after 1705 ostensibly ignore Indians, but they were automatically encompassed by the new definition of mulattoes. In 1715 and again in 1741 North Carolina curbed racial intermarriage. Drawing on the intent and almost verbatim on the blatantly racist language of Virginia's act of 1691, the Carolina legislators in 1741 sought to prevent an "abominable Mixture and spurious Issue" by levying a prohibitive fine against any white person who married "an Indian, Negro Mustee, or Mulatto Man or Woman, or any Person of mixt Blood, to the Third Generation, bond or free."⁵⁴ Other colonies did not explicitly prohibit Indian-white marriages before the American Revolution, although Massachusetts almost did in 1705. Samuel Sewall, a supporter of missionary efforts among the New England tribes, claimed to have thwarted the attempt.⁵⁵ In most colonies the taint of color prejudice and the cultural gulf were sufficient deterrents to intermarriage.

Comparable clues to changing Anglo-American perceptions of the Indian are laws concerning servants and slaves. In 1705 Virginia legislated protections for white servants by bracketing together the people it considered non-Christian and nonwhite: "[N]o Negroes, Mulattoes, or *Indians*, although Christians, or *Jews, Moors, Mahametans*, or other Infidels, shall, at any Time, purchase any Christian Servant, nor any other, except of their own Complexion." South Carolina in 1712 considered "all negroes, mulattoes, mustizoes or Indians" to be slaves unless they could prove otherwise; later in the century Georgia borrowed heavily from her colonial neighbor when prescribing slavery for "all negroes, Indians, mulattoes, or mestizoes, who now are, or hereafter shall be in this province (free Indians in amity with this government, and negroes, mulattoes, or mestizoes, who now are or hereafter shall become free, excepted)."⁵⁶

Even eighteenth-century New Englanders, with their sparse black and Indian populations, were almost as ready as their Southern counterparts to fuse the two groups in legislation. Cases in point are New Hampshire's "Act to Prevent Disorders in the Night" of 1714 and the regulations proposed in 1723 by Boston's selectmen to control its colored population. The New Hampshire legislation

⁵⁴ A. Leon Higginbotham, Jr., *In the Matter of Color: Race and the American Legal Process—The Colonial Period* (New York, 1978), 122, 123, 127, 129, 169, 204; Hening, *The Statutes at Large*, 86–88; *A Collection of All the Acts of Assembly, Now in Force, in the Colony of Virginia* (Williamsburg, Va., 1733), 222–23; and John D. Cushing, ed., *The Earliest Laws of North Carolina, 1669–1751*, 2 vols. (Wilmington, Del., 1977), 1: 130, and 2: 65. The only laws in the seventeenth century that commonly classed Indians with blacks were proscriptions on militia service.

⁵⁵ [Sewall] *The Diary of Samuel Sewall, 1674–1729*, ed. M. Halsey Thomas, 2 vols. (New York, 1973), 1: 532. Almon Wheeler Lauber stated that Massachusetts outlawed European marriages to Indians in 1692, and subsequent authors have relied on his testimony. Lauber cited no source, however, and I have not been able to find such an act in the existing records. Lauber, *Indian Slavery in Colonial Times within the Present Limits of the United States* (New York, 1913), 253. For an argument that, despite some restrictive legislation, Indians were not generally subjected to the same proscriptions as blacks, see Jordan, *White over Black*, 163. I concur, but more important in early eighteenth-century legislation is the trend toward equating the two groups. Moreover, champions of Indian-European miscegenation were scarce in the eighteenth century and did not practice what they preached. Symptomatically, perhaps, one of the most frequently cited voices for intermarriage, Robert Beverley in his *History and Present State of Virginia* of 1705, deleted such advocacy in his edition of 1722. Compare Beverley, *The History and Present State of Virginia* (London, 1705), ed. Louis B. Wright (Chapel Hill, 1947), p. 38, with the corresponding page of the edition of 1722, which does not contain the critical paragraph.

⁵⁶ *A Collection of All the Acts of . . . Virginia*, 220; Higginbotham, *In the Matter of Color*, 169; and Cushing, *Earliest Printed Laws of the Province of Georgia*, 2: 276.

empowered local officials and “Her Majesties good Subjects” to “Apprehend or Cause to be Apprehended any Indian, Negro, or Molatto, Servant or Slave that shall be found abroad after Nine a Clock at Night, and shall not give a good and Satisfactory Account of their business.” Boston’s “Articles for the Better Regulating Indians Negroes and Molattos within this Town” set appalling restrictions on non-European residents, whether slave, servant, or free. One provision, for example, required that “every free Indian Negro or Molatto Shal bind out, all their Children at or before they arrive at the age of four years to Some English master.”⁵⁷ That Indians shared with blacks and mulattoes such repressive policies in colonies as diverse as New Hampshire, Massachusetts, New York, Virginia, the Carolinas, and Georgia (others could be cited) speaks volumes about shifting Anglo-American racial perceptions.

By the eve of the American Revolution, Anglo-Americans had taken the next logical step: they defined “Americans” wholly in terms of themselves. Americans, James Otis assured readers of his *Rights of the British Colonies Asserted and Proved* (1764), were not “a compound mongrel mixture of *English, Indian, and Negro*, but . . . freeborn *British white* subjects.” Hector St. John de Crevecoeur agreed. “The American, this new man,” he explained in 1786, was a transplanted European or his descendant. A year later, John Jay expanded that notion in *The Federalist*, number 2: “Providence has been pleased to give this one connected country, to one united people, a people descended from the same ancestors, speaking the same language, professing the same religion, attached to the same government, very similar in their manners and customs.”⁵⁸ By the prevailing definition, Indians (and Negroes) simply were not Americans.

Nor, in the eyes of many Anglo-Americans, did Indians merit eventual incorporation into American society. Anglo-America’s fundamental contempt for Indian culture remained relatively constant throughout the history of British America and beyond. What changed under the influence of the new perception of Indians as innately dark-skinned were expectations of the Indians’ civil and theological redemption. Initially the Indians had been viewed as likely prospects for anglicization; by the middle of the eighteenth century they were usually dismissed as incapable and unworthy. Accompanying that judgment was a weakening of the few protections that Indians, as individuals or as tribes, had enjoyed in the previous century.

In the sixteenth and early seventeenth centuries, English optimism had served as a real, though grossly imperfect, shield against wholesale slaughter, enslavement, or flagrant abuse of the Indians, except in periods of outright war. Although all British colonies subjected Indians to some legal discrimination—prohibitions against serving in the militia or acquiring guns, horses, and liquor, for example—and to individual acts of insult or exploitation, colonial governments often

⁵⁷ A. S. Batchellor, ed., *Laws of New Hampshire*, 2 (Concord, 1913): 138–39; and *A Report of the Record Commissioners for the City of Boston, Containing the Boston Records from 1700 to 1728* (Boston, 1883), 173–75.

⁵⁸ Otis, *The Rights of the British Colonies Asserted and Proved* (Boston, 1764), 24; Crevecoeur, *Letters from an American Farmer* (New York, 1957), 39; and Jay, *The Federalist*, no. 2, ed. Jacob E. Cooke (Middletown, Conn., 1961), 9. The second edition of Crevecoeur’s *Letters* (London, 1783) differs from the first edition, and almost all subsequent editions, in stating that the American “is *neither* an European, *nor* the descendant of an European” (italics added); the essential message is, however, the same: that Americans are a blend of European stocks.

protected Indian rights and punished white transgressors.⁵⁹ Governmental concern stemmed partly from self-interest, of course: abuse of the Indians could bring lethal retaliation and costly wars. But in most colonies there were genuinely humane leaders who shared the widespread contempt for Indian culture but who also tried to treat individual Indians justly and peacefully. Because they considered the Indians to be as truly and as perfectly God's creatures as anyone, they counted on education and conversion to make Indians into Englishmen. Among the most solicitous of Indian welfare were George Thorpe in early Virginia, the second Lord Baltimore in Maryland, William Penn in Pennsylvania, and Edward Winslow and the elder Jonathan Mayhew in New England.⁶⁰ Most missionaries, too, sought to protect the Indians' lives and well-being while attempting—as missionaries by definition must—to alter the Indians' basic beliefs.

The protections afforded by government and church officials gradually faded during the seventeenth century as public sentiment turned increasingly cynical about the possibility of coexistence and ultimate conversion and as the swelling European-American population became increasingly difficult to regulate. By the mid-eighteenth century, the best the Indian could expect was seclusion on an ever-shrinking reservation or westward migration in the face of an advancing colonial frontier. Powerful tribes won some respect for their usefulness as allies or their danger as enemies; the rest, judging from the writings of the time, were considered by most Anglo-Americans a nuisance to be ignored, enslaved, or eliminated. As Benjamin Franklin observed in 1764 after a major atrocity in western Pennsylvania, "The Spirit of killing all Indians, Friends and Foes, [has] spread amazingly thro' the whole Country." Two years later, Sir William Johnson reported to Whitehall that, in the backcountry from Virginia to New York, the settlers "murder, Robb and otherwise grossly misuse all Indians they could find . . . and [are] treating the Indians with contempt, much greater than they had ever before experienced." The settlers, Sir William lamented, "perpetrate Murders whenever opportunity offers." The stereotypical frontier view that "the only good Indian is a dead Indian" was an American reality long before General Sheridan coined the infamous phrase.⁶¹

Even missionaries reflected the changing view of Indian color and character. During the eighteenth century a new wave of missionary enthusiasm revived efforts

⁵⁹ Although Britain's imperial and colonial governments were uneven and unpredictable in their defense of Indian rights in the seventeenth century, most passed laws against abuse of Indians and made at least sporadic attempts to enforce them. For a complex combination of reasons, Pennsylvania's record on this score is undoubtedly the best, early New England's a distant second best, and Virginia's clearly the worst. Historians are deeply divided on the relative merits of early Indian policies, but most would agree, I believe, that seventeenth-century colonial courts were more likely to punish Euro-American transgressors against Indians than were their eighteenth-century counterparts.

⁶⁰ Thorpe and Lord Baltimore have received relatively little attention from historians of Indian-European culture contact, the former because the information is scarce, the latter because Maryland's early ethnic history has been largely overlooked. Penn has fared far better, drawing frequent (and usually uncritical) praise for his attitudes. Winslow and Mayhew have figured prominently in historical debates over the Puritans' Indian policies; see note 69, below. For an argument that the degree of fairness in Puritan court decisions depended on expediency and ethnocentricity, see Lyle Koehler, "Red-White Power Relations: Justice in the Courts of Seventeenth-Century New England," *American Indian Culture and Research Journal*, 3, no. 4 (1979): 1–31.

⁶¹ Franklin to Richard Jackson, February 11, 1764 in Labaree *et al.*, *Papers of Benjamin Franklin*, 11: 76–78; and Johnson to Henry Conway, June 28, 1766, in E. B. O'Callaghan and Berthod Fernow, eds., *Documents Relative to the Colonial History of . . . New York*, 15 vols. (Albany, 1856–87), 7: 835–36.

in many colonies to Christianize the Indians, now that the danger from Indian hostility had receded and as the Great Awakening aroused Christian consciences throughout British America. In a resurgence of attempts to indoctrinate the Indians in English ways and beliefs, several new schools and colleges admitted native youths. Even among educators and institutions, however, the image of the Indian was less hopeful and more susceptible to racial—as distinct from cultural—bias than it had been in the previous century. Plans for Virginia's Indian College at Henrico in the early 1620s and the Harvard charter of 1650 had reflected the seventeenth century's expectation of rapid success based on the biological equality of Indians and Englishmen.⁶² Eighteenth-century efforts reveal a different perception.

The early career of Dartmouth College is illustrative. Several critics of Eleazar Wheelock's missionary school in Lebanon, Connecticut, the precursor to his college in Hanover, New Hampshire, attributed its slim success to English prejudice, freely admitting that they themselves "could never respect an Indian, Christian or no Christian, so as to put him on a level with white people on any account." One of the critics insisted, on the basis of "the irresistible aversion that white people must ever have to black [*sic*]," that "Mr. Wheelock's attempt [*is*] altogether absurd and fruitless. . . . [A]s long as the Indians are despised by the English we may never expect success in Christianizing of them." And it can hardly be coincidental that Wheelock frequently referred to his Indian students as "black" or to his prize pupil, Sansom Occom, as "my black son." Even some of Wheelock's students absorbed notions of abject inferiority and its related color terminology. One Mohegan called himself a "good for nothing Black Indian," and "a despicable Lump of polluted Clay . . . inclosed in this tawny skin."⁶³ The degradation of the Indians had, for Wheelock and his students at least, reached the point where native Americans were rhetorically almost indistinguishable from the Africans who had suffered a century and a half of enslavement and extreme prejudice. It is not, therefore, surprising that in 1764 Thomas Hutchinson of Massachusetts complained, "We are too apt to consider the Indians as a race of beings by nature inferior to us, and born to servitude."⁶⁴ By Hutchinson's time, thousands—perhaps tens of thousands—of Indians were, in fact, enslaved in British colonies from Massachusetts to Barbados. The eighteenth century's view of Indians as innately dark and inferior thus had frightful implications for America's aboriginal inhabitants.

⁶² On the proposed Virginia College, see Robert Hunt Land, "Henrico and Its College," *WMQ*, 2d ser., 18 (1938): 453–98. The Harvard College charter of 1650, which committed the institution "to the Education of the English, & Indian Youth of this Country," is reprinted in *Harvard College Records* (*Colonial Society of Massachusetts Publications*, 15 [1925]), 40–42; Harvard's Indian college is described in Samuel Eliot Morison, *Harvard College in the Seventeenth Century*, 2 vols. (Cambridge, Mass., 1936), 1: chap. 17.

⁶³ David Crosby to Wheelock, November 4, 1767, Dartmouth College Library, Hanover, N.H., Papers of Eleazar Wheelock, WP 767604.1; Wheelock to Dennys de Berdt, May 4, 1761, *ibid.*, WP 761304.1; Joseph Johnson to Wheelock, April 20, 1768, in James Dow McCallum, ed., *The Letters of Eleazar Wheelock's Indians* (Hanover, N.H., 1932), 131; and Johnson to Wheelock, December 4, 1774, *ibid.*, 183. On Wheelock's educational and racial views, see James Axtell, "Dr. Wheelock's Little Red School," chapter 4 of his *The European and the Indian: Essays in the Ethnohistory of Colonial North America* (New York, 1981).

⁶⁴ Hutchinson, *The History of the Colony of Massachusetts-Bay* . . . , 1 (Boston, 1765; reprint edn., New York, 1972): 283.

DOCUMENTING THE SHIFTS IN ANGLO-AMERICAN PERCEPTIONS of Indian color is easier than explaining them. Contemporary authors were oblivious to the changes; they were too close to the phenomena and too involved in them. To a large extent, of course, the reasons for changing attitudes can only be surmised, for they reflect a vast and complicated alteration in millions of disparate individuals whose perceptions of the Indian cannot be precisely reconstructed. Nonetheless, Anglo-American writings of the eighteenth century offer important clues to the psychological imperatives that encouraged "white" Americans to believe that Indians were significantly and irrevocably darker than themselves. At least three major interrelated and mutually reinforcing influences are apparent: the Anglo-Americans' anger at Indian hostility, their frustration over Indian rejection of Christianity and "civility," and their adoption of eighteenth-century racial theories.

First, chronologically, was the transformation of the Indian in English eyes from potential friend to inveterate enemy. That change took place gradually and unevenly, occurring at different times in different places, as military conflict increasingly characterized Indian-English contact. From the standpoint of Anglo-American attitudes, the causes of conflict were irrelevant: Englishmen at home or in America almost invariably blamed the Indians for hostilities and, hence, came to think of them as incorrigibly aggressive and ruthless.⁶⁵ It was only a short step from regarding the Indians as bloodthirsty foes to perceiving them as naturally inferior in morality and humanity, and eventually in color.

The initial Anglo-American view of the Indians was largely amiable and optimistic. Most of the "savages," imperial spokesmen contended, would be friendly. They would also be eager for commerce and the gospel, partly because it would be in their own self-interest but mainly because the English professed to come peacefully, offering voluntary acceptance of English culture and religion. (By contrast, English imperialists viewed Spanish settlement as a model of how to alienate and exterminate the natives.) The Indians would therefore welcome English outposts, willingly sell surplus land, engage in mutually profitable trade, and enthusiastically embrace Protestantism. Yet, from the earliest days of English colonialism, its champions predicted—judging from a century of European experience in America—that some natives would oppose settlement no matter how fairly they were treated. And Indians who persisted in hostility or obstinately rejected free trade and proselytizing, the imperialists argued, deserved no quarter; the English never seriously questioned their own right to occupy part of America, by force if necessary. England's deeply ingrained ethnocentrism (a characteristic other European nations possessed, but apparently to a lesser degree), and England's determination to make profits and converts, which it hoped would emulate Spanish success but not Spanish methods, would brook no native opposition. Even the usually benign younger Hakluyt minced no words on this point: "To handle them gently, while gentle courses may be found to serve . . . be without comparison the

⁶⁵ For an argument that the English mind was especially susceptible to seeing the Indians—or anyone—as untrustworthy, see Karen Ordahl Kupperman, "English Perceptions of Treachery, 1583–1640: The Case of the American 'Savages,'" *Historical Journal*, 20 (1977): 263–87.

best: but if gentle polishing will not serve, then we shall not want hammerours and rough masons enow, I meane our old soldiours trained up in the Netherlands, to square and prepare them to our Preachers hands." With the few recalcitrants chastised, colonization would proceed peacefully, to the benefit of settlers and Indians alike.⁶⁶

Such expectations died early. At Roanoke Island in the 1580s, most of the Indians turned against the colonists, for justifiable reasons, and the neighboring Powhatans probably exterminated the "Lost Colony" of 1587.⁶⁷ In Virginia, settlers and natives clashed almost incessantly from 1607 to 1613, often in open warfare, sometimes in sporadic skirmishes, occasionally in bloodless but hostile negotiations. Nearly a decade of relative calm followed the captivity and conversion of Pocahontas in 1613; the brief and imperfect respite from hostilities ended suddenly with the massacre of 1622, which almost exterminated the colony. Even though responsibility for the massacre ultimately belonged with the English, as their own accounts unwittingly reveal, the Anglo-American attitude toward the Indians quickly shifted from contempt to hatred—a sentiment intensified by the ensuing decade of blatant carnage and by a similar massacre in 1644.⁶⁸

New England's experience offers some parallels and some marked contrasts to Virginia's. Unlike Virginia, the New England colonies had generally peaceful relations with the local tribes until 1675. To a considerable extent that reflected New England's unusual population ratio: epidemics in 1616–17 and 1633–34 greatly reduced the natives while barely touching the colonists, which lessened Indian resistance and further encouraged Puritan immigrants to believe that God intended them to create a New English Zion. So did the Pequot War of 1637, which briefly threatened New England's view of the Indians as potential friends and converts. The decisive victory over the Pequots further reduced Indian numbers, and, when most of the tribes remained neutral or actively supported the colonists, the Puritans' confidence in their own invincibility and in the Indians' vulnerability was reinforced rather than undermined.⁶⁹ By contrast, early Virginians faced an

⁶⁶ Early imperialistic views of America include Crashaw, *Sermon Preached in London*; [Robert Johnson] *Nova Britannia: Offering Most Excellent Fruits by Planting in Virginia* (London, 1609); and Taylor, *Writings and Correspondence of the Two Richard Hakluyts*, 1: 118, and 2: 334, 502–03. For the younger Hakluyt on "gentleness," see *ibid.*, 2: 503.

⁶⁷ On the Roanoke experience, see Quinn, *The Roanoke Voyages, 1584–1590*, 1: 270–72, 281–82, 284–88. For a recent discussion of the Lost Colony's fate, see David N. Durant, *Raleigh's Lost Colony* (New York, 1981), chaps. 17–18.

⁶⁸ On early Indian-English relations in Virginia, see Bernard Sheehan, *Savagism and Civility: Indians and Englishmen in Colonial Virginia* (Cambridge, 1980); Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), esp. chaps. 2–6; J. Frederick Fausz, "The Powhatan Uprising of 1622: A Historical Study of Ethnocentrism and Culture Contact" (Ph.D. dissertation, College of William and Mary, 1977); and Alden T. Vaughan, "'Expulsion of the Salvages': English Policy and the Virginia Massacre of 1622," *WMQ*, 3d ser., 35 (1978): 57–84. Significantly, at this early stage of hostilities, Anglo-Americans did not apply color terminology. Even Edward Waterhouse, the most vitriolic of the massacre historians, described the Indians as "naked, tanned, deformed Savages"; Susan Myra Kingsbury, ed., *The Records of the Virginia Company of London*, 4 vols. (Washington, 1906–35), 3: 557.

⁶⁹ Indian affairs in early New England have been seen from widely divergent perspectives. Compare Alden T. Vaughan, *New England Frontier: Puritans and Indians, 1620–1675* (rev. edn., New York, 1979), with Francis Jennings, *The Invasion of America: Indians, Colonialism, and the Cant of Conquest* (Chapel Hill, N.C., 1975), pt. 2, and Richard Drinnon, *Facing West: The Metaphysics of Indian-Hating and Empire Building* (New York, 1980), pt. 1.

Indian population that vastly outnumbered them and that had enough political cohesion to use its numerical strength effectively.

But numbers do not tell the whole story. Important too was the strong missionary impulse among New England's founders, an impulse that eventually enjoyed a quarter-century of modest achievement. New England's missionary activity began belatedly in the 1640s, but its success from then until King Philip's War seemed to justify earlier expectations. The uprisings of 1675—once again an Indian response to colonial encroachment and abuses—dashed Puritan assumptions about the eventual transformation of the Indians into proper Englishmen. Henceforth, the British colonists in New England joined those in the Chesapeake and elsewhere in a growing conviction that Indians in general were their enemies. That conviction hardened in the late seventeenth century and throughout the eighteenth as the British colonial frontier became a vast English-Indian battleground, often exacerbated by troops or agents from other colonial powers. Even Pennsylvania, after the effective withdrawal of the Quakers from political control and after the influx of a predominantly non-Quaker population, had its share of racial conflict.⁷⁰

Frequent and ferocious hostilities, regardless of who was at fault, inevitably corroded the earlier Anglo-American view of the Indians and reshaped its vocabulary. References to the Indians, never especially flattering, now became almost universally disparaging. In the aftermath of 1622, Anglo-American spokesmen portrayed the Virginia natives as "having little of Humanitie but shape," "more brutish than the beasts they hunt," and "naturally born slaves."⁷¹ New Englanders reacted similarly to King Philip's War. The Indians were "Monsters shapt and fac'd like men," wrote one New England poet, and most of his compatriots undoubtedly agreed.⁷² Even book titles reflect the shift in attitude. In 1655 John Eliot could write hopefully of the Indians' progress toward conversion in *A Late and Further Manifestation of the Progress of the Gospell amongst the Indians in New England: Declaring Their Constant Love and Zeal to the Truth, with a Readiness to Give Account of Their Faith and Hope, as of Their Desire to Be Partakers of the Ordinances of Christ*. Twenty years later, in the midst of New England's struggle for survival, an anonymous pamphleteer suggested a far different view of the Indians in a *Brief and True Narration of the Late Wars Risen in New-England, Occasioned by the Quarrelsome Disposition, and Perfidious Carriage of the Barbarous, Savage, and Heathenish Natives there*.⁷³ As warfare increasingly became the dominant mode of English-Indian contact, the image of the Indian as vicious savage made deep inroads on the Anglo-American psyche.

⁷⁰ The history of military conflict is traced in a number of secondary studies, most notably in Douglas Edward Leach, *Arms for Empire: A Military History of the British Colonies in North America, 1607–1763* (New York, 1973); Howard H. Peckham, *The Colonial Wars, 1689–1762* (Chicago, 1964); and John E. Ferling, *A Wilderness of Miseries: War and Warriors in Early America* (Westport, Conn., 1980). For Pennsylvania's shift from peaceful relations to frequent hostilities, see Francis Paul Jennings, "Miquon's Passing: Indian-European Relations in Colonial Pennsylvania, 1674–1755" (Ph.D. dissertation, University of Pennsylvania, 1965), pt. 4. After 1755, the pace of conflict quickened.

⁷¹ Purchas, *Hakluytus Posthumus; or, Purchas His Pilgrimes*, 19: 231; and John Boneil, *His Majesties Gracious Letter to the Earle of South-Hampton* . . . (London, 1622), 26.

⁷² Benjamin Thompson, *New Englands Crisis* (Boston, 1676), 19.

⁷³ Eliot's *Late and Further Manifestation of the Progress of the Gospell amongst the Indians* (London, 1655) is reprinted in *Massachusetts Historical Society Collections*, 3d ser., 4 (1834): 261–87. The anonymous tract appeared in Boston in 1675.

Cotton Mather, whose rhetorical flights often exaggerated but seldom misrepresented colonial sentiments, gave revealing advice to New England's soldiers in King William's War: "Once you have but got the Track of those Ravenous howling Wolves, then pursue them vigourously; *Turn not back till they are consumed. . . . Beat them small as the Dust before the Wind. . . . Sacrifice them to the Ghosts of Christians whom they have Murdered. . . . Vengeance, Dear Country-men! Vengeance upon our Murderers.*" The culmination of a century and a half of military escalation came in 1776 in the Declaration of Independence's only reference to the Indians: The king "has endeavored to bring on the inhabitants of our frontiers, the merciless Indian savages, whose known rule of warfare is an undistinguished destruction of all ages, sexes and conditions." Nearly a decade of border warfare exacerbated the revolutionaries' fear and hatred of the Indians. "The white Americans," observed a British traveler in 1784, "have the most rancorous antipathy to the whole race of Indians; and nothing is more common than to hear them talk of extirpating them totally from the face of the earth, men, women, and children."⁷⁴

War-bred animosities did not require a difference in color perception, but the unconscious temptation to tar the Indian with the brush of physical inferiority—to differentiate and denigrate the enemy—appears to have been irresistible.⁷⁵ War-time epithets have often invoked outward appearance, however irrelevant (witness the "yellow Japs" of World War II), and British Americans frequently resorted to pejorative color labels. In the late seventeenth and early eighteenth centuries, as war raged along the northern New England frontier, Cotton Mather castigated "those Tawny Pagans, than which there are not worse Divels Incarnate upon Earth," and "a *swarthy* Generation of *Philistines* here; the *Indian* Natives, I mean, whom alone we are like to have any *Warrs withal*." Nearly a century later, when the bulk of the Indians sided with Great Britain during the American Revolution, Henry Dwight complained of "copper Colour'd Vermine" and hoped that an American army would "Massacre those Infernal Savages to such a degree that [there] may'nt be a pair of them left, to continue the Breed upon the Earth."⁷⁶ Logically enough, "redskins" eventually emerged as the epithet for enemies who usually used red paint on the warpath. Not coincidentally, perhaps, the first reported use of that term appears in a passage about Indian assaults on frontier settlements. In a sentence that suggests the impact of war on changing English attitudes, Samuel Smith of Hadley, Massachusetts, recalled in 1699 that several decades earlier his father had endured Indian raids in the Connecticut Valley. "My

⁷⁴ Mather, *Souldiers Counsell'd and Comforted: A Discourse Delivered unto Some Part of the Forces Engaged in the Just War of New England against the Northern and Eastern Indians* (Boston, 1689), 28; Julian P. Boyd, ed., *The Papers of Thomas Jefferson*, 1 (Princeton, 1950): 431; and J. D. F. Smyth, *A Tour of the United States of America*, 2 vols. (London, 1784), 1: 345–46.

⁷⁵ On the tendency to stress differences rather than similarities, see Margaret T. Hogden *Early Anthropology in the Sixteenth and Seventeenth Centuries* (Philadelphia, 1964), 196–201. Despite virulent English antipathy toward the Irish during the same period, however, the Irishman's whiteness was incontrovertible; hence, the English could not perceive of him as dark-skinned. Moreover, the Irishman seldom stained or painted his skin, as the Indians did, so that the outward appearance of the former remained white, of the latter tawny or red. But note the implications of the epithet "black Irishman."

⁷⁶ Mather, *Fair Weather: or, Considerations to Dispel the Clouds, and Allay the Storms of Discontent . . .* (Boston, 1692), 86, and *Military Duties, Recommended to an Artillery Company, at Their Election of Officers . . .* (Boston, 1687), 22; and Henry William Dwight to Theodore Sedgwick, February 18, 1779, Massachusetts Historical Society, Boston, Sedgwick Papers.

Father ever declar'dt," Smith remembered, "there would not be so much to feare iff ye Red Skins was treated with suche mixture of Justice & Authority as they cld understand, but iff he was living now he must see that wee can do nought but *fight* em & that right heavily."⁷⁷

The Indians' refusal to adopt English concepts of civility and religion poisoned Anglo-American attitudes as thoroughly as did warfare. Sixteenth- and seventeenth-century expectations of rapid and wholesale anglicization met constant rebuff; by the end of the seventeenth century it must have been clear to all but the more optimistic missionaries that most Indians would never be Christian in faith or English in allegiance and customs. Converts in the southern and middle colonies numbered only a handful; most of John Eliot's "praying towns" had been scuttled by King Philip's War and its aftermath; and even the Quakers in Pennsylvania, despite a commendable effort to treat the Indians fairly, had won few to English ways or beliefs. Occasional successes notwithstanding, the missionary movement had failed. Even less successful was the broader mission of eliminating customs that Englishmen subsumed under the heading of "savagery," such as nakedness, scarification, tribal law and government, hunting instead of herding, and, perhaps most important of all, an exclusively oral language. Some technological assimilation had occurred, as had some imaginative blending of religious ideas, but the overwhelming majority of Indians steadfastly held to their traditional ways and rejected most of the alien culture's offerings.⁷⁸

Who was to blame? The most obvious scapegoat was the Indian himself. He stubbornly resisted spiritual and material improvement, his critics charged, and they eventually concluded that his resistance stemmed either from a deeply ingrained antipathy to "civilization" or from a natural incapacity for improvement. Benjamin Franklin explained to a correspondent in 1753 that "Little Success . . . has hitherto attended every attempt to Civilize our American Indians in their present way of living. . . . When an Indian Child has been brought up among us, taught our language and habituated to Our Customs, yet if he goes to see his relations and make one Indian Ramble with them, there is no perswading him ever to return." Franklin did not consider the Indians inherently incapable of adopting English ways; they simply and obstinately preferred their own.⁷⁹ Many of Franklin's contemporaries were less charitable. The editor of the 1764 edition of William Wood's *New England's Prospect*, for example, thought the Indians incurably barbarian and pagan. "The christianizing the Indians," he peevishly noted,

scarcely affords a probability of success; for their immense sloth, their incapacity to consider abstract truth . . . and their perpetual wanderings, which prevent a steady worship, greatly impede the progress of Christianity, a mode of religion adapted to the most refined temper of the human mind. . . . The feroce manners of a native Indian can

⁷⁷ Smith to Ichabod Smith, 1699, in Helen Evertson Smith, *Colonial Days and Ways, as Gathered from Family Papers* (New York, 1900), 50.

⁷⁸ There is no way to measure precisely the number of anglicized Indians, but by all accounts it was small. For an attempt to compute the number in New England, see Alden T. Vaughan and Daniel K. Richter, "Crossing the Cultural Divide: Indians and New Englanders, 1605–1763," *Proceedings of the American Antiquarian Society*, 90 (1980): 23–46. On the importance of language to early European imperialists, see Stephen J. Greenblatt, "Learning to Curse: Aspects of Linguistic Colonialism in the Sixteenth Century," in Fredi Chiappelli, ed., *First Images of America: The Impact of the New World on the Old*, 2 (Berkeley and Los Angeles, 1976): 561–80.

⁷⁹ Franklin to Peter Collinson, May 9, 1753, in Labaree et al., *Papers of Benjamin Franklin*, 4: 481.

never be effaced, nor can the most finished politeness totally eradicate the wild lines of his education.

Almost predictably, the editor believed that Indians were not born white: with few exceptions, contempt for Indians correlated highly with a belief in their innate darkness.⁸⁰

A third major influence toward perceiving the Indians as inherently tawny or red came from eighteenth-century naturalists. Few of them had first-hand information about the American Indians—most were European scholars who never visited the New World—but in their frantic attempt to classify systematically all plant and animal life, including the principal divisions of mankind, they contributed directly to the notion of Indians as inherently red and indirectly at least to the belief in their inferiority.

Initially, the naturalists' categories had no hierarchical intent. Their taxonomies were horizontal, not vertical, and each branch of humanity enjoyed equality with all others. Before long, however, the subdivisions of *homo sapiens* acquired descriptive judgments that suggest a relative superiority in Europeans and corresponding inferiority in other races. Such a view meshed perfectly with the eighteenth century's emphatic belief in natural order, metaphorically expressed as a "Great Chain of Being," in which all creatures from microorganisms to angels had permanent places on a hierarchical continuum. The idea of an orderly chain of life had existed for centuries; it flourished in the fifteenth century, for example, when Sir John Fortescue recorded a classic description: "In this order angel is set over angel, rank upon rank in the Kingdom of Heaven; man is set over man, beast over beast, bird over bird, and fish over fish . . . so that there is no worm that crawls upon the ground, no bird that flies on high, no fish that swims in the depths, which the chain of this order binds not in most harmonious concord."⁸¹ Not until the eighteenth century, however, did ranks *within* humankind receive much attention. Then, because natural scientists almost invariably chose skin color as the principal criterion of racial identity, darkness of hue became "scientifically" linked to other undesirable qualities. As Winthrop Jordan has pointed out, for Africans the "Great Chain of Being" soon became a "Great Chain of Color" on which whites regarded blacks as divinely relegated to a lesser rank of humanity.⁸² In the eighteenth

⁸⁰ Wood, *New England's Prospect*, ed. Rogers, 94. Belief in Indian inferiority—in *person* as well as in culture—did not always go hand in hand with a perception of Indians as inherently tawny or red, but the correlation is high. One could argue endlessly over a list of European-Americans who believed the Indians to be their potential equals (after a cultural change), but a consensus could probably be reached on William Penn, Roger Williams, John Eliot, and James Adair; by contrast, *racial* prejudice seems to underlie the attitudes of Nathaniel Rogers, Bernard Romans, and the essayists in *DeBow's Review* (see below). Thomas Jefferson is, as always, eminently arguable. Compare Jordan, *White over Black*, esp. 453, 477–81, and Drinnon, *Facing West*, esp. 78–116. On this point, I find Drinnon more convincing than Jordan. Most of Jefferson's biographers have lauded his attitude toward Indians.

⁸¹ [Fortescue] *The Works of Sir John Fortescue, Knight, Chief Justice of England . . .*, ed. Thomas Fortescue, 1 (London, 1869): 322. On eighteenth-century naturalists and the definition of race, see Bentley Glass *et al.*, eds., *Forerunners of Darwin, 1745–1859* (Baltimore, 1959); Clarence J. Glacken, *Traces on the Rhodian Shore: Nature and Culture in Western Thought from Ancient Times to the End of the Eighteenth Century* (Berkeley and Los Angeles, 1967); and Benjamin Keen, *The Aztec Image in Western Thought* (New Brunswick, N.J., 1971). The landmark discussion of the "Great Chain of Being" is Arthur O. Lovejoy, *The Great Chain of Being: A Study of the History of an Idea* (Cambridge, Mass., 1936).

⁸² Jordan, *White over Black*, 252–59.



Figure 3: Charles Linnaeus. Swedish naturalist Linnaeus's classifications and color labels had profound impact on European and American perceptions of the Indians. Reproduced from D. J. N. Stoeber, *The Life of Sir Charles Linnaeus*, trans. Joseph Trapp (London, 1794).

century, American Indians also became victims, though not quite so pejoratively, of the color chain's invidious implications.

The second edition of Charles Linnaeus's *General System of Nature* (1740) presented the Swedish botanist's preliminary attempt at human classification. The previous edition (1735) had said almost nothing about mankind; the new and greatly enlarged version gave only a few lines to the subject, but they were enough to establish four basic groups: "Europaeus albus, Americanus rubescens, Asiaticus fuscus, Africanus niger." Seven subsequent editions, published between 1740 and 1756, repeated those color categories verbatim. In the tenth edition (1758), Linnaeus expanded his discussion of humans. To the four original categories he added two others—wild men and monsters, to whom he assigned no colors—while again ascribing white, red, yellow, and black to Europeans, Americans, Asians, and Africans. But Linnaeus no longer restricted his entries to physical description; he now attributed several character traits to each race. Europeans were, among other things, sanguine, brawny, gentle, and inventive; Americans choleric, obstinate,

content, and free; Asians melancholy, rigid, haughty, and covetous; Africans phlegmatic, crafty, indolent, and negligent.⁸³ The Indians, to be sure, fared far better than Africans and Asians in this Linnaean glossary, and Linnaeus even hinted at the Noble Savage image (“content,” “free”) that was rapidly gaining favor among European intellectuals. But an unavoidable message in Linnaeus’s highly subjective and immensely influential treatise was that American Indians were naturally red and somewhat inferior to whites.

Most of Linnaeus’s scientific contemporaries concurred. In 1744, four years after Linnaeus’s first description of Americans as red, Dr. John Mitchell of Virginia investigated “the proximate cause of the Colour of *Negroes*, Indians, white People, etc.” Drawing on Newton’s *Opticks*, Mitchell argued that the thickness of the skin determined the amount of light reflected by the epidermis and, hence, the extent of its darkness. He saw all mankind as varying only in shade—the Indians differed from Europeans in degree rather than substance—yet even Mitchell argued that the Indians of America and Asia (whom he considered of about the same color) comprised a distinct race and a distinct color category. He also contended that tawny was the original human color: Africans had become darker, Europeans lighter, “Americans retaining the primitive and original Complexion.”⁸⁴ Perhaps from a desire to defend native Americans from European charges of physical inferiority, Mitchell thus gave considerable dignity to Indian color. In that regard, Mitchell was unique.

Of the two prominent eighteenth-century authors who differed significantly with Linnaeus on the Indians’ natural hue, one preceded him by almost two decades and admitted sparse knowledge of America. Richard Bradley’s *A Philosophical Account of the Works of Nature* (1721) lists Indians among the world’s white-skinned people. Of his five categories of mankind, which Bradley based on hair texture as well as on skin color, he included in “the *White Men*” two subcategories: “*Europeans*, that have *Beards*; and a sort of *White Men in America* (as I am told) that only differ from us in having no *Beards*.”⁸⁵ Bradley represents a waning stage in the perception of Indian complexion. More distinguished than Bradley, and more notorious for the counterattacks he provoked, was Georges Louis Leclerc, Comte de Buffon. The French scientist’s *Natural History* appeared in several editions and innumerable volumes (forty-four in the 1749 version) and was hailed as one of the great works of the time. His disparaging remarks on America in general and on the Indians in particular, however, aroused Jefferson’s ire and inspired other critics as well. Some attacked Buffon for his contention that all mankind was naturally white, “which may be varied by climate, by food, and by manners, to yellow, brown, and black . . . Nature, in her most perfect exertions, made men white.” This extreme environmentalism prodded Henry Home, Lord Kames, to issue an extensive critique. Kames took Buffon to task on several counts, including the notion that

⁸³ Linnaeus, *General System of Nature*, as quoted in T[homas] Bendyshe, “The History of Anthropology,” *Memoirs of the Anthropological Society of London*, 1 (1863–64): 422, 424–25; and Jordan, *White over Black*, 220–21.

⁸⁴ Mitchell, “An Essay upon Causes of the Different Colours of People,” *Philosophical Transactions of the Royal Society*, 43 (1744–45): 120–30, 146–59.

⁸⁵ Bradley, *A Philosophical Account of the Works of Nature* (London, 1721), 169.

climate is the principal cause of color. If that were true, Kames asked, why were “all Americans without exception . . . of a copper-colour, though in that vast continent there is every variety of climate?”⁸⁶

No answer was really needed, however, for Buffon—much like James Adair at a later date—was simultaneously on both sides of the argument. Although Buffon attributed human color variations to environmental influence, he believed them to be almost permanent: centuries of exposure to a certain climate and to certain foods and body stains could produce essentially different races. And, although Buffon recognized that American Indian complexions varied considerably, he argued that “the whole continent of America contains but one race of men, who are all more or less tawny.”⁸⁷ In sum, while Buffon disagreed sharply with Kames and others about the fundamental cause of racial colors, he nonetheless perceived the Indians almost exactly as his critics did—innately dark and racially distinct. That perception appeared with particular virulence on the eve of the American Revolution in Bernard Romans’s *Concise Natural History of East and West Florida* (1775). “From one end of America to the other,” Romans contended, “the red people are the same nation and draw their origin from a different source, than . . . any other different species of the human genus . . . [They are] a people not only rude and uncultivated, but incapable of civilization.”⁸⁸ The overwhelming thrust of eighteenth-century naturalism thus supported the growing conviction that American Indians were inherently deficient in color and basic characteristics. Science had joined forces with war-bred animosities and missionary disillusionment to brand the Indians with an ineradicable mark of inferiority.

IT IS BEYOND THE SCOPE OF THIS ESSAY to trace in detail the evolution of white America’s attitudes and policies toward Indians in the early national and antebellum periods. The story has recently received extensive scholarly attention and undoubtedly will continue to attract analysis and debate.⁸⁹ But a brief overview will suggest the major trends and relate them to changing color perceptions.

⁸⁶ Leclerc, Comte de Buffon, *Natural History, General and Particular, by the Count de Buffon*, trans. William Smellie, 9 vols. (2d ed., London, 1785), 3: 57, 173–78, 181, 200–03; and Home, Lord Kames, *Six Sketches of the History of Man* (Philadelphia, 1776), 14–16.

⁸⁷ Buffon, *Natural History*, 3: 57, 173–74, 188. Historians often divide early theorists of the Indians’ physical attributes into environmentalists, who believed that climate and circumstances determined color and culture, and biblicists, who viewed basic characteristics as divinely ordained and hence immutable. Some “environmentalists” certainly deserve the label; but others expected changes to occur only after centuries, or at least several generations, and thus were not really admitting that any of their dark-skinned contemporaries (or even the children of their contemporaries) could achieve a light pigmentation and all it implied in racial acceptance. Samuel Stanhope Smith is a case in point. Environment, he contended, could change most aspects of human appearance and behavior, but “a black or dusky complexion, once contracted by the ancestors of a race, is continued in their offspring by a much lower climactical influence, than was originally necessary to create it.” Smith thought North American Indians to be naturally dark but made more so “by discolouring paints and unguents.” Smith, *An Essay on the Causes of the Varieties of Complexion and Figure in the Human Species*, ed. Winthrop D. Jordan (Cambridge, Mass., 1965), 94–95, 108, 113, 176–77.

⁸⁸ Romans, *Concise Natural History of East and West Florida*, 38–39.

⁸⁹ Significant studies in the last two decades include Francis Paul Prucha, *American Indian Policy, 1790–1834* (Cambridge, Mass., 1964); Reginald Horsman, *Expansion and American Indian Policy, 1783–1812* (East Lansing, Mich., 1967), and “Scientific Racism and the American Indian in the Mid-Nineteenth Century,” *American Quarterly*, 27 (1975): 152–68; Richard Drinnon, *White Savage: The Case of John Dunn Hunter* (New York, 1972);

American racial thought in the post-Revolutionary era underwent subtle but significant changes. Ever since the sixteenth century, Europeans had viewed Africans and Indians as fundamentally different from each other in postdiluvian biological development and in their prospects for absorption into British-American society. In the eighteenth century, the gap gradually closed as the Indian became, in the eyes of most white observers, inherently tawny. As earlier, most Anglo-Americans considered Africans immutably black; in the late eighteenth and early nineteenth centuries some writers further debased the Afro-American by contriving polygenic theories to explain what they believed to be the black race's irredeemable physical and behavioral inferiority. Indians, by contrast, were sometimes lauded for superior appearance, virtue, or ability, yet increasingly they too suffered the stigma that white America attached to peoples of darker skin.⁹⁰

That the Indian was, in fact, inherently darker than the European, and that his pigmentation was the sign of a separate branch of mankind, had become axiomatic by the outbreak of the American Revolution. What remained for the Jeffersonian generation and its early nineteenth-century successors was to determine the Indians' proper color label and to reach a rough consensus on the implications of a racial status that was clearly inferior to the white man's but also superior to the black's.

The precise reason for the gradual adoption of red, instead of tawny or some other hue, can only be surmised. Although the earliest recorded use of "redskins" dates from the very late seventeenth century, it is an isolated example and its authenticity is slightly suspect. (Samuel Smith's letter of 1699, which uses "Red Skin Men" and "Redskins," was not published until 1900, when it appeared as a purported transcription inserted in a descendant's Revolutionary-era diary; it may reflect a later editorial hand.) Not until the second half of the eighteenth century did "red" emerge as a fairly common label. By 1765 some Indians may have adopted the primary color label for themselves, either in a pan-Indian sense or

Bernard Sheehan, *Seeds of Extinction: Jeffersonian Philanthropy and the American Indian* (Chapel Hill, N.C., 1973); Robert Berkhofer, *The White Man's Indian: Images of the American Indian from Columbus to the Present* (New York, 1978); Hoover, *The Red and the Black*, chaps. 3–4; and William McLoughlin, "Red Indians, Black Slavery, and White Racism: America's Slaveholding Indians," *American Quarterly*, 26 (1974): 365–85, and "Cherokee Anomie, 1794–1809: New Roles for Red Men, Red Women, and Black Slaves," in Richard L. Bushman *et al.*, eds., *Uprooted Americans: Essays to Honor Oscar Handlin* (Boston, 1979), 125–60. Also relevant and important is Robert E. Bieder, "The American Indian and the Development of Modern Anthropological Thought in the United States, 1780–1851" (Ph.D. dissertation, University of Minnesota, 1972). Reginald Horsman's excellent *Race and Manifest Destiny: The Origins of American Racial Anglo-Saxonism* (Cambridge, Mass., 1981) appeared too late for incorporation in this essay. On most matters Horsman and I seem to agree, although I am more impressed than he appears to be with the pervasiveness of popular antipathy to the Indians in the last quarter of the eighteenth century, and I am less impressed than he by the Founding Fathers' supposedly enlightened ideas on race. I think their actions, and indeed many of their statements, reveal them to hold essentially the same racist notions and to be only slightly less pessimistic about the Indians than were their more sanguinary contemporaries. Horsman and I also differ in terminology. I see no advantage to his substitution of *racialism* for *racism*; both terms describe the belief that inferior character and ability are inherited by members of certain biologically distinct—by color or otherwise—branches of humankind. I see no significant difference between today's racism and that of the late eighteenth and early nineteenth centuries.

⁹⁰ Benjamin Rush attributed African pigmentation to hereditary leprosy, which, he believed, could be cured by long residence in temperate climates; Rush, *Sixteen Introductory Lectures, to Courses . . . [on the] Practice of Medicine* (Philadelphia, 1811). Rush is often hailed as one of the Revolutionary era's most enlightened spokesmen, but his statements on race are hardly praiseworthy. For example: "A dull and disgusting sameness of mind characterizes all savage nations," which "is as much the effect of the want of physical influence upon their minds, as a disagreeable colour and figure are of its action upon their bodies." *Ibid.*, 93–94.

perhaps in reference to a particular tribe: "We, red people," a native speaker asserted, "are a very jealous people." (That quotation, too, did not appear in print until much later.) By the 1760s the continuing association of red bodypaint and an almost perpetual enmity—later heightened by the War for Independence—made "redskin" the most plausible epithet. Linnaeus's use of red gave additional impetus. Although the reasons for his choice remain obscure, most likely he sought a primary color to parallel the other races' black, white, and yellow. Red was the obvious candidate because Indians used red stains so widely and because it avoided the confusion that might have come from colors such as brown, olive, and tawny, which were sometimes applied to racial subgroups, especially to North Africans and mulattoes and occasionally to European ethnic groups. In any event, Jefferson and some of his contemporaries used "red" as a racial category in the 1770s and 1780s, a trend that spread rapidly. "Indians," "natives," and "savages" continued to be common appellations, but the conviction sown in the third quarter of the eighteenth century that Indians were inherently red took firm root during the next half century. In the middle of the nineteenth century, James Fenimore Cooper's *The Redskins* (1846) and anthropologist Henry Rowe Schoolcraft's *The Red Race in America* (1847) symbolically marked Caucasian America's full recognition, in both fiction and science, of Indians as innately red and racially distinct.⁹¹

Once red became a viable designation, it seems to have satisfied everyone. To the Indians' bitterest critics, red could signify ferocity, blood, and anger; to their most avid supporters and to the Indians themselves, red could suggest bravery, health, and passion; to those who fell between the judgmental extremes, red could mean almost anything or nothing. In short, red was sufficiently flexible and ambiguous to meet the metaphysical imperatives of a society that did not wholly agree about the Indian's basic character or social and political fate.

The emerging perception of the Indians as innately different and inferior was widespread but not universal. Had it been universal, missionary activity would have languished or expired everywhere; most Indians would have been enslaved or exterminated; and the use of Indians as symbols of America would have stopped abruptly. None of these happened. Despite abundant instances of wholesale disregard for Indian rights and welfare, the late eighteenth and early nineteenth centuries witnessed a lesser but significant countertrend in attitudes toward the

⁹¹ Smith, *Colonial Days and Ways*, 48–50; and Extract from the Journal of George Croghan in S[amuel] P[rescott] Hildreth, *Pioneer History: Being an Account of the First Examination of the Ohio Valley . . .* (Cincinnati, 1848), 79. As early as 1753, a colonist wrote of "Red Men," but there are few similar references before 1775, when such terminology suddenly proliferated. John O'Neal to James Hamilton, May 27, 1753, in Samuel Hazard, ed., *The Register of Pennsylvania*, 4 (Philadelphia, 1829): 389. Cooper, in his various novels, used color terminology extensively, including "redskins" and "palefaces." In *The Wept of Wish-ton-Wish: A Tale*, 2 vols. (New York, 1829), 61–62, Cooper made clear his belief in the permanence of Indian color when Ruth describes an Indian as a "creature, formed, fashioned gifted like ourselves, in all but color of the skin and blessing of the faith." Nathaniel Hawthorne and Herman Melville, along with most other American novelists, used "red men" or its variants. By the nineteenth century, most historians, including Francis Parkman, George Bancroft, and William H. Prescott, also referred to "redskins" and "red men." For an analysis of their views on Indians, see David Levin, *History as Romantic Art: Bancroft, Prescott, Motley, and Parkman* (Stanford, Calif., 1959), esp. 127–58. Fictional accounts are analyzed in Albert Keiser, *The Indian in American Literature* (New York, 1933). Linnaeus's impact is hard to measure, but certainly his works were well known to leading eighteenth-century American intellectuals. Cadwallader Colden, among others, corresponded with Linnaeus. Jefferson cited him, and Charles Wilson Peale named his second son Charles Linnaeus.

Indians that afforded them some protection and kept the hostile view from wholly controlling Indian policy in the new nation.

Hints of the Indian as a noble savage had appeared in the earliest decades of European contact with America, but the concept did not reach major stature until the middle of the eighteenth century in Europe and the early nineteenth century in America. Largely as a reaction against what they considered the corruptions of their own societies, European *philosophes* sought models of the natural simplicity and order from which “civilized” society supposedly had fallen in its insatiable quest for property and power. Two attractive foils were available: the “Golden Age” of antiquity and aboriginal America. Both were widely used, and in the process the virtues of both were exaggerated. That was perhaps inevitable when writing of the Indians because none of the *philosophes* had first-hand knowledge of Indian life; in striking contrast to Buffon, they idealized Indian society, although, like Buffon’s, their interpretation served their political and intellectual needs. A few European-Americans, especially along the eastern seaboard, adopted similar sentiments.⁹² They and others who respected Indian culture—traders and missionaries, for example, whose personal acquaintance with Indians tempered their ethnocentricity, or systematic students of Indian life, such as William Bartram and John Heckwelder—largely resisted the groundswell of racist attitudes toward the Indians. Although pro-Indian sentiment was a minority viewpoint, it had important repercussions: it encouraged the political, educational, and missionary endeavors of the Revolutionary and early national periods that Bernard Sheehan has labeled “Jeffersonian philanthropy.”⁹³

The noble savage countertrend in American thought insisted that certain Indian characteristics were laudable in their own right and—more important—were especially admirable when compared to contemporary European society. Because the Indians seemed to live an unfettered and unacquisitive life amid unspoiled nature, they were hailed by some Americans (following the *philosophes*) as the ideal to which modern man should, to some extent at least, return. Because the Indians appeared to be a “vanishing race,” they were romanticized in history, art, and literature. Because the Indians were America’s most truly native inhabitants, they personified the nation in statues, cartoons, and cartouches.⁹⁴

⁹² The standard treatment of the noble savage theme is Hoxie N. Fairchild, *The Noble Savage: A Study in Romantic Naturalism* (New York, 1928); also useful is A. L. Dicket, “The Noble Savage Convention as Epitomized in John Lawson’s *A New Voyage to Carolina*,” *North Carolina Historical Review*, 43 (1966): 413–29. Definitions of noble savagism—usually implicit—vary widely. Too often the term is applied to European observations that merely praise Indian stature or certain traits of character such as hospitality or hardiness; but such praise was almost universal, even among the Indians’ most vehement detractors. The true noble savage perception of the Indian, I submit, rests on a belief in (1) the superiority of Indian culture in general to European or Anglo-American culture and (2) the degeneracy of Western culture from an earlier, “higher” level. The *philosophes* saw American Indians as noble because they had not yet succumbed to the corruptions—both individual and collective—of eighteenth-century life. The Indian remained in a Golden Age that closely approximated an earlier European stage of development.

⁹³ Sheehan, *Seeds of Extinction*. On the Indian in Jeffersonian thought, also see Daniel J. Boorstin, *The Lost World of Thomas Jefferson* (New York, 1948), chap. 2. For Bartram’s view of the Indians, see *The Travels of William Bartram* (1791), ed. Francis Harper (New Haven, 1958), esp. 306; and, for Heckwelder’s views, see his *History, Manners, and Customs of the Indian Nations . . .*, *Memoirs of the Pennsylvania Historical Society*, no. 12 (Philadelphia, 1876).

⁹⁴ Europeans had long used Indian motifs to represent the American continent in plays and illustrations. Americans were slower to adopt such symbols but increasingly used them in the Revolutionary era. Post-



AMERICA

Figure 4: "America." By the early nineteenth century, many Anglo-Americans saw Indians as neither Noble Savages nor symbols of the American continent. Rather, as this English picture from 1804 suggests, Minerva now symbolized America while the Indian had merged with the African into a minor motif. Reproduced courtesy of The Henry Francis du Pont Winterthur Museum, Winterthur, Del.

Even among their supposed supporters, however, the Indians fared badly, as a host of examples from Jefferson through Thomas McKenney and Lewis Cass amply illustrate. Such “friends” of the Indians contended that in the long run the Indian must be incorporated into the American mainstream, but increasingly in the nineteenth century the number of such advocates declined and the duration of the Indians’ expected tutelage expanded. The Indians, assimilationists argued, must become farmers, landowners, and citizens; they must adopt “white” America’s language, laws, and customs. They must cease to be Indians⁹⁵—even if it took centuries to reach that end. Along the way, Indian desires were rarely considered. Although the overwhelming majority wanted to retain their land and their culture, the Eastern tribes had little choice. In the first half of the nineteenth century, the basic options were assimilation or extermination, with removal to the West as a temporary stage in either case.

Some assimilationists advocated miscegenation as the surest path to de-Indianization. Ironically, the biological solution emerged almost simultaneously with laws in many states against Indian-white intermarriage.⁹⁶ Such laws hint at hypocrisy and racial intransigence among the majority of Americans and reveal the implausibility of intermarriage as a solution to race relations in nineteenth-century America. But whatever the solution—miscegenation, allotment of farmlands in the East, removal to the West, or education in white-controlled boarding schools—the Indian was marked for gradual extinction by the uneasy coalition of his friends and foes. For both groups, as well as for the bulk of Americans who were neither friend nor foe but merely indifferent, the conviction that the Indians were innately and ineradicably “redmen” underlay their concern, or lack of it, for the Indians’ fate. And to most white observers—certainly to the Indians’ foes and almost certainly to the millions who scarcely cared—the stereotypical color carried a host of unfavorable associations that prevented the Indians’ full assimilation into the Anglo-American community and simultaneously precluded their acceptance as a separate

Revolutionary America turned from Indian iconography toward classical symbols and eventually to “Columbia” as a national representation. Significantly, in nineteenth-century paintings that retained an Indian figure, he or she was often assigned to the background and given unmistakably African features and color—a naked and feathered Indian-black child (see figure 4). Relevant studies include James H. Hyde, “The Four Parts of the New World in Old-Time Pageants and Ballets,” *Apollo*, 4 (1926): 232–38, and 5 (1927): 19–27; Roy C. Strong, *Splendor at Court: Renaissance Spectacle and the Theatre of Power* (Boston, 1973); E. McClung Fleming, “The American Image as Indian Princess, 1765–1783,” *Winterthur Portfolio*, 2 (1965): 65–81, and especially “From Indian Princess to Greek Goddess: The American Image, 1783–1815,” *ibid.*, 3 (1967): 37–66; and Hugh Honor, *The New Golden Land: European Images of America from the Discoveries to the Present Time* (New York, 1975).

⁹⁵ For the paradoxical views of the Indians’ early nineteenth-century supporters, see the works cited in note 92, above. On McKenney, also see Herman J. Viola, *Thomas L. McKenney: Architect of America’s Early Indian Policy, 1816–1830* (Chicago, 1974).

⁹⁶ Massachusetts provides a prime example of the impact of emergent racism on antimiscegenation laws. It had no restrictions against Indian-English marriages in the seventeenth century (although, for cultural reasons, few occurred); in 1705 it almost prohibited such marriages but did not, finally, until 1786. Yasuhide Kawashima, “Forced Conformity: Puritan Criminal Justice and Indians,” *Kansas Law Review*, 25 (1977): 362 n. 5. (Kawashima’s reference to an antimiscegenation act of 1692 is based on Lauber, *Indian Slavery in Colonial Times*; see note 55, above.) Two other New England states proscribed Indian-white marriages in the late eighteenth and early nineteenth centuries; see *The Public Laws of the State of Rhode-Island and Providence Plantations* (Providence, 1798), 483; and *Laws of the State of Maine* (Portland, 1822), 276. The Maine law of 1821 drew verbatim on the Massachusetts act of 1786.

and equal people. Although a few dissenters resisted the prevailing color taxonomy and its correlative racial policies, the surviving literature, both factual and fictional, shows that the Indian was no longer considered a member of the same race; he remained forever distinct in color and character. Even relatively sympathetic spokesmen now believed the Indians to be permanently different. "No Christianizing," declares Natty Bumppo in Cooper's *Pathfinder* (1840), "will ever make even a Delaware [Indian] a white man, nor any whooping and yelling convert a paleface into a red-skin." Bumppo earlier—in *The Prairie* (1827)—puts the same idea more succinctly: "Red natur' is red natur'."⁹⁷

By the middle of the nineteenth century, the shift in Anglo-American perception had reached its logical conclusion: Indian culture was merely a reflection of primordial racial shortcomings and impervious to education or missionization. The Indian was inherently deficient in *character*, which his color proclaimed for all to see. In 1850 the influential *United States Magazine and Democratic Review* could assert as indisputable the "fact that the dark races are utterly incapable of attaining to that intellectual superiority which marks the white race." Four years later, *DeBow's Review* epitomized the antebellum South's attempt to blend racism, ethnography, and polygenesis into a bulwark of white supremacy. "The doctrine of the Unity of the Race, so long believed by the world," one writer insisted, "is ascertained to be false. We are not all descended from one pair of human beings. . . . The negro till the end of time will still be a negro, and the Indian still an Indian." Nor should the white race meddle with a situation beyond its control. Another essayist in *DeBow's* drew the obvious lesson from nearly a century of racist thought: The Indian's "race is run, and probably he has performed his earthly mission. He is gradually disappearing, to give place to a higher order of beings. The order of nature must have its course."⁹⁸ A popular pseudoscientific study, *Types of Mankind* (1854), was less philosophical: "[I]t is in vain to talk of civilizing [the Indians]. You might as well attempt to change the nature of the buffalo." Given such nineteenth-century sentiments, based on a pervasive perception of Indians as inherently dark, incurably savage, and intrinsically non-American, the Indians' subsequent plight comes as no surprise.

⁹⁷ Cooper, *The Pathfinder: or, The Inland Sea*, ed. Richard Dilworth Rust (Albany, N.Y., 1981), 122, and *The Prairie: A Tale*, 1 (Paris, 1827): 113. An English visitor to the United States in the 1790s voiced his pejorative judgment that the Christianized Stockbridge Indians had been "civilized as much as Indians are capable of civilization"; William Strickland, *Journal of a Tour in the United States of America, 1794–1795*, ed. J. E. Strickland, Collections of the New-York Historical Society, no. 83 (New York, 1971), 116. Cooper's corollary, that no white man could become an Indian any more than a redskin could become a white, was another important reflection of the new racial concepts. In the seventeenth century, Anglo-American spokesmen acknowledged (and warned against) the possibility of colonists becoming thoroughly Indianized. No biological barriers were perceived; the settler had only to exchange his ways for those of the Indian. For discussions of Englishmen who "went savage" in British North America, see James Axtell, "The White Indians of Colonial America," *WMQ*, 3d ser., 32 (1975): 55–88; and Vaughan and Richter, "Crossing the Cultural Divide," 46–99.

⁹⁸ *United States Magazine and Democratic Review*, new ser., 27, no. 145 (July 1850): 48; *DeBow's Review*, 17, no. 1 (February 1854): 69; *ibid.*, 16, no. 2 (February 1854): 147–48; and J. C. Nott and George R. Gliddon, *Types of Mankind* (Philadelphia, 1854), 69. Although such views culminated in the 1850s, they had been widespread for at least several decades. In 1830, for example, a writer predicted that "Civilization is destined to exterminate [the Indians], in common with the wild animals"; Charles Caldwell, *Thoughts on the Original Unity of the Human Race* (New York, 1830), 142. Such rhetoric scarcely differs, I contend, from the prevailing sentiment at the end of the American Revolution, although few intellectuals or statesmen were ready to speak so bluntly.

“Barbarous Strangers”: Hessian State and Society during the American Revolution

CHARLES INGRAO

ALTHOUGH THE STRUGGLE BETWEEN FREEDOM AND AUTHORITY has always been a popular theme among American historians, no example has inspired us more than our own experience in the War of Independence. In comparing American with English society at the outbreak of the Revolution, we have never lost sight of the picture first drawn by the patriots themselves of New World equality and freedom challenging the inequities of a typical Old World mercantilist oligarchy. Yet perhaps the greatest contrast with American values is provided by the government and society of Hesse-Cassel, the German state that supplied the British with most of their German mercenaries. In the last two centuries, we have moved little from the image first projected by Mercy Warren of the Hessians as “barbarous strangers” who committed atrocities against the American freedom fighters but who were themselves the “slaves” of a typically corrupt and greedy despot. Contemporary American historians have been somewhat more sympathetic to the mercenaries themselves, portraying them as naive and oppressed peasants, “simple farming people” who were either unwilling or unknowing pawns of their sovereign.¹

There has, however, been far less reinterpretation of the regime that dispatched them to America. Only in this century have we acknowledged the bogus origins of Benjamin Franklin’s *Uriasbrief*, in which the Hessian landgrave Frederick II purportedly decried the death of “only” 1,465 of his subjects at Trenton because the British paid him appreciably more for soldiers killed in action than for those who were merely wounded or captured.² Contemporary American historians still portray him as a greedy “petty despot” who sought British subsidies in order to finance a decadent court life and the needs of his one hundred bastard offspring. In retelling the old but unfounded story of the landgrave’s bastards, respected

A fellowship from the Alexander von Humboldt Foundation enabled me to do the research for this article. I also benefited from the advice of those of my colleagues who read and commented on it—namely, Donald J. Berthrong, Lester Cohen, John Contreni, Mark Edwards, Richard Haywood, Darlene Clark Hine, Robert May, Jim Oakes, Linda Levy Peck, Gunther Rothenberg, William Stueck, Jon Teaford, Philip VanderMeer, Harold Woodman, and Lamont Yeakey at Purdue University, T. C. W. Blanning at Cambridge University, Gregory Pedlow at the University of Nebraska, and James A. Vann at the University of Michigan.

¹ Warren, *History of the Rise, Progress, and Termination of the American Revolution*, 1 (Boston, 1805): 278, 283; Don Higginbotham, *The War of American Independence* (New York, 1971), 131; and John R. Alden, *The American Revolution, 1775–1783* (New York, 1954), 67.

² Philipp Losch, *Soldatenhandel* (Kassel, 1974), 37–55, and “Der Uriasbrief des Grafen von Schaumburg,” *Hessische Chronik*, 2 (1913): 37–40, 83–88, 99–105; and Karl E. Demandt, *Geschichte des Landes Hessen* (Kassel, 1972), 282. In fact, only about twenty Hessians were killed at Trenton.

historians have catered to the well-worn clichés that fit comfortably into the standard American view of European monarchy during the Old Regime.³

Only with the appearance in English of two new studies by European historians Ernst Kipping and Rodney Atwood have we been exposed to the judgment already prevalent among German scholars that the Hessian monarch was not the oppressive despot and the mercenaries themselves not the downtrodden and reluctant warriors we had supposed; they were rather the product of a society with values largely distinct from the one then emerging in America.⁴ These contributions make a start at revising a heretofore perfunctory and culturally biased view of the Hessian regime and its people. A close examination of Hesse-Cassel during the Revolutionary era does indeed point to considerable contrasts between two Western societies, but it also uncovers much common ground, including the dynamic role played in each society both by the parallel development of Enlightenment and Protestant mass ideology in shaping a political ethos and by active representative institutions in carrying it out. Although "the distinctiveness of the political culture in which the Revolutionary generation operated" cannot be denied,⁵ late eighteenth-century Hesse-Cassel and America were nevertheless two nations whose cultural and institutional development stemmed in part from common roots but whose own distinct circumstances ultimately led them to take up different philosophies of government—and opposing sides in the conflict.

IN DEALING WITH THE BROAD CURRENT OF ENLIGHTENMENT THOUGHT, each society selectively fixed upon and utilized those ideas that were most congenial to its own political environment. Hence, Enlightenment ideology was allowed not only to reshape but also to reinforce existing attitudes and institutions. Although most Western observers have tended to ignore the German *Aufklärung* as a "poor imitation" of its French and British counterparts, there has never been any question that Enlightenment ideas were firmly implanted in Germany by the second half of the eighteenth century. Largely through the influence of Christian Wolff, natural law and philosophy were taught at most German universities, especially those in the Protestant principalities.⁶ Wolff personally facilitated their introduction inside Hesse-Cassel. After Frederick William I had expelled him from Prussia in 1723 for espousing "atheism," he taught at Marburg for seventeen years. Although he

³ Higginbotham, *The War of American Independence*, 130–31; Alden, *The American Revolution*, 66–67; and Edward Jackson Lowell, *The Hessians and the Other German Auxiliaries of Great Britain in the Revolutionary War* (New York, 1884), 5.

⁴ Kipping, *The Hessian View of America, 1776–1783* (Monmouth Beach, N.J., 1971); and Atwood, *The Hessians: Mercenaries from Hessen-Kassel in the American Revolution* (Cambridge, 1980). Most notable in promoting a more positive image of the Hessian regime are Otto Berge, "Die Innenpolitik des Landgrafen Friedrich II. von Hessen-Kassel" (Ph.D. dissertation, University of Mainz, 1952); Wolf von Both and Hans Vogel, *Landgraf Friedrich II. von Hessen-Kassel* (Kassel, 1973); and Karl Hermann Wegner *et al.*, *Aufklärung und Klassizismus in Hessen-Kassel unter Landgraf Friedrich II., 1760–1785* (Kassel, 1979).

⁵ Gordon S. Wood, *The Creation of the American Republic, 1776–1787* (Chapel Hill, 1969), viii.

⁶ Peter Hanns Reill, *The German Enlightenment and the Rise of Historicism* (Berkeley and Los Angeles, 1975), 1; Hajo Holborn, *A History of Modern Germany, 1648–1840* (New York, 1969), 164; and Frederick Hertz, *The Development of the German Public Mind*, 2 (London, 1962): 226. For detailed treatment of the *Aufklärung*, see Peter Pütz, *Die deutsche Aufklärung* (Darmstadt, 1978); and Fritz Valjavec, *Die Entstehung der politischen Strömungen in Deutschland, 1770–1815* (Düsseldorf, 1951), 13–88.

returned to his old position at Halle following the accession of Frederick the Great in 1740, he left a lasting imprint on the Hessian university system. Both Marburg and its sister university at Rinteln came to be dominated by his former students or by other *Wolffianer*.⁷ Wolff's ideas were not lost on the state bureaucrats, almost all of whom were required to have attended a Hessian university as a condition for government employment. Nor were the new rationalist ideas alien to the future landgrave, Frederick. Educated at Geneva, he was instructed in natural law and philosophy by several of its professors, including Burlamaqui, one of Europe's leading popularizers of Enlightenment thought. After completing his formal education, he continued to read widely in the writings of the *philosophes* and, like so many of his princely contemporaries, corresponded with Voltaire and other French luminaries. His awareness and public acceptance of much of the Enlightenment creed is evident in "Pensées sur les princes et les ministres," which he wrote shortly after his accession.⁸

Yet, if Enlightenment thought had made significant inroads among the Hessian ruling elite, their perception of it was tailored to conform to the needs of established institutions. As in the rest of Germany, Hesse's *Aufklärung* was shaped by the nation's almost universal acceptance of cameralist monarchy, which was one of the most popular and highly centralized in the empire, and Christianity, which people still supported, valuing their country's historic role in the growth and defense of German Protestantism. On the whole, Enlightenment thought was not theoretically incompatible with the institution of monarchy. Although Western observers have often been inclined to see the *philosophes* as endorsing democratic institutions to the exclusion of other political systems, the *philosophes* were actually less concerned with the form of government than with its social thrust and capacity for removing the anachronisms and injustices of eighteenth-century society. It was in this vein that men like Voltaire, Diderot, and the physiocrats spoke for the majority of their contemporaries in endorsing absolutism as a viable avenue to reform.⁹

The flow of Enlightenment ideas was, of course, sufficiently broad and diverse to

⁷ Wilhelm Dersch, "Beitrag zur Geschichte der Universität Marburg im Zeitalter der Aufklärung," *Zeitschrift des Vereins für hessische Geschichte und Landeskunde* [hereafter, *ZhG*], 54 (1924): 165; H. Hermelink and S. A. Kaehler, *Die Philipps-Universität zu Marburg, 1527–1927* (Marburg, 1927), 387; Rudolf Schmitz, *Die Naturwissenschaften an der Philipps-Universität Marburg, 1527–1977* (Marburg, 1978), 43, 49; and Franz Karl Theodor Piderit, *Geschichte der hessisch-schaumburgischen Universität Rinteln* (Marburg, 1842), 129–30.

⁸ Alfred Cobban, *In Search of Humanity: The Role of the Enlightenment in Modern History* (New York, 1960), 162–63; Berge, "Innenpolitik des Landgrafen Friedrich II.," 5–6; Charles Borgeaud, *Histoire de l'Université de Genève* (Geneva, 1900), 510–18; Both and Vogel, *Friedrich II. von Hessen-Kassel*, 10–12; and Edmund Stengel, "Die Briefwechsel Voltaires mit Landgraf Friedrich II. von Hessen," *Zeitschrift für neufranzösische Sprache und Literatur*, 1 (1879), and 7 (1885). Frederick's correspondence with Voltaire and his confidante Mme. Gallatin is located in the Hessian State Archives, Marburg [hereafter, *StAMg*], 4a) 91, 44–45. Among other things, the "Pensées" extols the virtues of education and of free enterprise, advocates religious toleration and the elimination of corporal punishment, and acknowledges the primary obligation of government to serve the popular welfare. Frederick II, "Pensées sur les princes et les ministres," *StAMg*, 4a) 90, 12.

⁹ John G. Gagliardo, *Enlightened Despotism* (New York, 1967), 17–18; Leo Gershoy, *From Despotism to Revolution, 1763–1789* (New York, 1944), 67; Fritz Hartung, *Enlightened Despotism* (London, 1957), 6; Leonard Krieger, *An Essay on the Theory of Enlightened Despotism* (Chicago, 1975), 42, 69, 80; and Cobban, *In Search of Humanity*, 165. Folkert Hensmann has demonstrated that the physiocrats were not philosophically committed to a specific political system but nonetheless supported absolutism because they appreciated its potential for successfully carrying out their economic program; Hensmann, *Staat und Absolutismus im Denken der Physiokraten* (Frankfurt, 1976).

allow the American elite to justify its own revolt against Great Britain. Although much of the Revolutionary rhetoric stemmed from pre-Enlightenment Opposition Theory and did not delve deeply into the socioeconomic thought of eighteenth-century natural law, American radicals could still clothe their arguments in the political theories of Locke and Montesquieu.¹⁰ Yet, unlike either Locke or Montesquieu or even the Americans themselves, the German intelligentsia generally did not have to deal with an unpopular or widely discredited regime. If anything, many Germans prided themselves on the contrast between the desperate situation in France and their own smoothly functioning and "enlightened" governments.¹¹ Thus, while they occasionally criticized individual princes, the *Aufklärer* rarely felt compelled to question the efficacy of absolutism itself. If they were introspective in any sense, it was in their reappraisal of imperial institutions, not of the empire's individual hereditary monarchies. Moreover, the theoretical compatibility between absolutism and Enlightenment ideas was in many cases supplemented by a practical symbiosis between the rulers and the *Aufklärer* themselves. The great majority of German thinkers were either government bureaucrats, university faculty, or both, and were invariably rewarded for their service with good salaries and high social prestige. Hence, in contrast with France, Great Britain, or America, where most writers were either financially independent of the state (and often destitute) or sustained in their criticism by their loyalty either to a powerful opposition political faction or to a corporate body, most *Aufklärer* were bound to their government by their personal interests and, probably, by their own identification with the regime as well.¹²

This acceptance of absolutism was common to virtually all German political theorists. Like their French contemporaries, they asserted that natural law bound the monarch by a social compact with his subjects that enjoined him from violating their right to happiness and required him to devote himself to serving the popular welfare. Once he had accepted these limits and responsibilities, however, all of the most prominent German thinkers—from Pufendorf and Leibniz through Thomasius and Wolff to the young Kant—recognized the primacy of the central authority without allowing for any significant checks. Even later political thinkers like Schlözer and Moser and men of letters like Lessing, Wieland, and Schiller, who occasionally criticized individual princes and stressed a ruler's obligations to society, never questioned the advantages of absolutism or advocated effective limits on royal authority.¹³ Meanwhile, the competing ideas of Locke and Montesquieu had only

¹⁰ Bernard Bailyn, *The Ideological Origins of the American Revolution* (Cambridge, Mass., 1967), 198–229, 302; and Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 369.

¹¹ Rudolf Vierhaus, "Deutschland im 18. Jahrhundert," in Franklin Kopitsch, ed., *Aufklärung, Absolutismus, und Bürgertum in Deutschland* (Munich, 1976), 187.

¹² T. C. W. Blanning, "The German Problem in the Eighteenth Century," chapter 1 of his *Reform and Revolution in Mainz, 1743–1803* (Cambridge, 1974), 22–23; Klaus Epstein, *The Genesis of German Conservatism* (Princeton, 1966), 51; and Hans-Joachim Braun, "Economic Theory and Policy in Germany, 1750–1800," *Journal of European Economic History*, 4 (1975): 312. For a closer examination of the symbiosis of crown and government bureaucracy, see Wilhelm Bleek, *Von der Kameralausbildung zum Juristenprivileg* (Berlin, 1972).

¹³ Leonard Krieger, *The German Idea of Freedom* (Chicago, 1957), 53–68, 71–74, 77–78, 89–90; Hans von Voltolini, "Die naturrechtlichen Reformer und die Reformen des 18. Jahrhunderts," *Historische Zeitschrift*, 105 (1910): 76–77; Blanning, "The German Problem in the Eighteenth Century," 17, 21–22; Holborn, *Modern*

limited impact within Germany. Although numerous *Aufklärer* leaned heavily on Hobbes's *Leviathan*, they invariably overlooked Locke's political writings in favor of his work on education. By the same token, they tended to de-emphasize Montesquieu's advocacy of a division of power; indeed, more than one writer actually used his *De l'esprit des lois* to advocate the virtues of absolutism.¹⁴

If German political thought endorsed absolutism, it had even less difficulty extolling the virtues of the highly structured bureaucratic state, or *Polizeistaat*, another institution that had preceded the Enlightenment but that the *Aufklärung* easily integrated into the realm of natural law. Like absolutism itself, the tightly run *Polizeistaat* was also congenial with the thrust of Enlightenment thought, if only because the German cameralists who advocated it envisioned government as yet another science that could be mastered by the minds of men for the benefit of society. Although not all *Aufklärer* were cameralists, few perceived an alternative to the valuable service performed by the hard-working bureaucracy. Moreover, given the close ties between academicians and government officials, the *Polizeistaat* enjoyed the same community of interests that rendered German absolutism immune from attack. Far from opposing Christian Wolff's advocacy of government direction of all human institutions, the *Aufklärer* tended to concur with J. H. G. Justi's vision of the state as a machine that, like Kepler's universe, could function with flawless precision once set in motion by some sovereign power.¹⁵

A second distinctive characteristic of the *Aufklärung* was its greater acceptance of Christianity and the church establishment. The absence of strong anticlerical sentiment was especially evident in Protestant societies, like that of Hesse-Cassel, on both sides of the Atlantic. Notwithstanding Frederick William's charge of "atheism," Christian Wolff contributed to the integration of revealed with natural religion. As had Pufendorf, Leibniz, and Thomasius before him, Wolff stressed the compatibility of natural law with Divine Providence and the possibility of fusing human reason with revelation. Through this synthesis, educated Germans were able to accept natural religion while retaining Christianity as the vehicle of their religious faith. As a result, within Germany as in America, deism remained an "exotic plant" that rarely took root and never flourished.¹⁶ The impact of this compromise was evident

Germany, 1648–1840, 162; Geraint Parry, "Enlightened Government and Its Critics in Eighteenth-Century Germany," *Historical Journal*, 6 (1963): 180–81; Roy Pascal, *The German Sturm und Drang* (Manchester, 1963), 42; Paul Hazard, *European Thought in the Eighteenth Century* (New York, 1963), 179–80; G. P. Gooch, *Germany and the French Revolution* (London, 1965), 23; Vierhaus, "Deutschland im 18. Jahrhundert," 184–85; and Epstein, *The Genesis of German Conservatism*, 257.

¹⁴ Klaus P. Fischer, "John Locke in the German Enlightenment: An Interpretation," *Journal of the History of Ideas*, 36 (1975): 438, 441; Rudolf Vierhaus, "Montesquieu in Deutschland," in *Collegium Philosophicum: Studien Joachim Ritter zum 60. Geburtstag* (Stuttgart, 1965), 419–34; Paul Bernard, *Jesuits and Jacobins: Enlightenment and Enlightened Despotism in Austria* (Urbana, Ill., 1971), 16; and Blanning, "The German Problem in the Eighteenth Century," 24.

¹⁵ Parry, "Enlightened Government and Its Critics," 182–84; and Blanning, "The German Problem in the Eighteenth Century," 18. For two recent examinations of the *Polizeistaat* as a pre-Enlightenment phenomenon, see Reinhold Dorwart, "The Theory of the Welfare State," chapter 1 of his *The Prussian Welfare State before 1740* (Cambridge, Mass., 1971), 1–29; and Marc Raeff, "The Well-Ordered Police State and the Development of Modernity in Seventeenth- and Eighteenth-Century Europe: An Attempt at a Comparative Approach," *AHR*, 80 (1975): 1221–43.

¹⁶ Voltolini, "Die naturrechtlichen Reformer und die Reformen," 86–87; Holborn, *Modern Germany, 1648–1840*, 161; Perry Miller, *Nature's Nation* (Cambridge, Mass., 1967), 110; and Henry F. May, *The Enlightenment in America* (New York, 1976), 71, 73, 122–26, 136–40.

within Hesse. Although the Marburg faculty refused to tolerate the teaching of deism, it had accepted the notion of religious "relativism" by the 1770s.¹⁷ Following his return from Geneva, the future landgrave, Frederick, completed his religious instruction with two thick volumes written by the same author—one entitled *La Religion naturelle*, the other *La Religion révélée*. The philosophical "exercises" that he wrote shortly before his accession stress his conviction of the equal justification of other religions.¹⁸

Inside Hesse and the rest of Protestant Germany, the integration of Christianity with natural law was aided by a second phenomenon, albeit one that was not only independent of the early *Aufklärer* like Wolff but even antagonistic to them. Pietism emerged as a mass movement in early eighteenth-century Germany and colonial America. German Pietism was closely related to its New World cousin not just in its theological but also in its moral and social message, stressing both greater self-discipline, industry, and duty and a heightened sense of social responsibility, particularly with regard to public charity.¹⁹ American historian Perry Miller pointed to European parallels to the Great Awakening and Pietism in the colonies, but his greater familiarity with conditions in the Anglo-Saxon world led him to assume that, since popular religion was generally at odds with Enlightenment thought, it was not embraced by Europe's educated elite and remained merely a movement of the masses.²⁰ Leading Pietist thinkers, such as August Hermann Francke, did oppose German rationalism and even secured Wolff's expulsion from Halle; yet the two movements occupied some common ground, including the concern for serving the popular welfare and promoting religious toleration. Moreover, Pietist influence was so pervasive in its infiltration of schools, churches, and families that it continued to exercise a strong influence over German Protestants, regardless of their subsequent exposure to Enlightenment ideas. Although the *Aufklärer* were not themselves Pietists, they remained "activated by Pietism" and never fully discarded its message; perhaps more important, university-educated bureaucrats also retained their Pietist values, even while accepting the new rationalist ideas.²¹

The pervasiveness of Pietistic ideas among the educated elite did more than facilitate the coexistence of Enlightenment and Christian values within the *Aufklärung*; it also helped lead Protestant Germans further away from their American coreligionists in the realm of politics. In the New World the mass Protestant revival played a major role in mobilizing popular support against Great Britain. In calling for men to cleanse themselves and society of their sins by defying the tyrannical and

¹⁷ Hermelink and Kaehler, *Philipps-Universität zu Marburg*, 401, 406.

¹⁸ StAMg, Handschriften, 91–93; and Englische Übungen, nos. 65, 104, 122, 4a) 90, 17. This kind of confessional relativism eventually weakened the prince's own Calvinist faith to the point that he converted to Roman Catholicism for its ceremonial pomp. Both and Vogel, *Friedrich II. von Hessen-Kassel*, 15, 25; and Eduard Vehse, *Geschichte der deutschen Höfe* (Hamburg, 1853), 167.

¹⁹ G. Uhlhorn, *Die christliche Liebestätigkeit*, volume 3: *Seit der Reformation* (Stuttgart, 1890), 262–72; Heinrich Hepp, *Kirchengeschichte beider Hessen*, 2 (Marburg, 1876): 327–38; and Friedrich Klingender, "Der Pietismus in Hessen-Kassel" (Ph.D. dissertation, University of Göttingen, 1919).

²⁰ Miller, *Nature's Nation*, 83.

²¹ Carl Hinrichs, *Preussentum und Pietismus: Der Pietismus in Brandenburg-Preussen als religiös-soziale Reformbewegung* (Göttingen, 1971); Reill, *The German Enlightenment*, 6; and Blanning, "The German Problem in the Eighteenth Century," 27–28.

corrupt mother country, Pietism employed both the vocabulary and thrust of popular religion to galvanize the American public against government authority in the cause of freedom—or, more specifically, decentralization and local autonomy.²² By contrast, in the highly centralized *Polizeistaaten* like Hesse-Cassel, which already enjoyed a strong tradition of government activism, the sense of social responsibility inspired by Pietism impelled the *Aufklärer* and bureaucrats alike to become more involved in regulating society as they sought to prod people into exercising greater self-discipline in eschewing their natural tendencies toward lethargy and hedonism. With Wolff himself preaching that it was the role of government to ensure that people “fulfill their duties toward themselves, toward others, and toward God,” the *Aufklärung* and its minions in the government utilized Pietistic currents to promote greater state control for the good of society.²³ Hence, Enlightenment ideals and mass Protestant ideology that were mobilized to justify greater freedom on one side of the Atlantic were marshaled for greater government control on the other.

IF PRE-EXISTING POLITICAL STRUCTURES and religious currents helped shape the thrust of the *Aufklärung* in Germany and Hesse, there were still other conditions endemic to German society, but especially exaggerated in Hesse-Cassel, that reinforced government activism.

Even by German standards Hesse-Cassel was poor. Agriculture was handicapped by a hilly, heavily wooded terrain, generally infertile soil, and an inhospitable climate. At the same time, the nearly four hundred thousand Hessians in 1781 saddled the country with a crushing population density of about one hundred and twenty people per square mile. While the peasants suffered most from these conditions, even many nobles lived modestly and were generally no more wealthy (and frequently were poorer) than the average American freeholder. Conditions were no better in the cities and towns. Although Hesse had once been a major textile center, it had never fully recovered from the devastation of the Thirty Years' War; by the mid-eighteenth century, its overall commercial-industrial middle class was still small, even for Germany.²⁴ Contemporary travelers passing through Hesse-Cassel usually commented on the wretchedness of its provincial towns and cities. Even the university center and one-time *Residenzstadt* of Marburg invariably appalled its visitors, including the widely traveled Wilhelm von Humboldt, who termed it “easily the ugliest and most unpleasant city” one could ever imagine.²⁵

The economic outlook was especially gloomy at Frederick's accession in 1760.

²² Harry Stout, “Religion, Communications, and the Ideological Origins of the American Revolution,” *William and Mary Quarterly*, 3d ser., 34 (1977): 530–35, 540–41; Perry Miller, “From the Covenant to the Revival,” in J. W. Smith and A. L. Jamison, eds., *The Shaping of American Religion* (Princeton, 1961), 322–68, and *Nature's Nation*, 90–107, 110; and Alan Heimert, *Religion and the American Mind* (Cambridge, Mass., 1966), 388–90, 460–63, 512–19.

²³ Krieger, *The German Idea of Freedom*, 68; and Dorwart, *The Prussian Welfare State before 1740*, 17–18.

²⁴ Menschen- und Viehverzeichnis de anno 1781, StAMg, 5) 10597; George Thomas Fox, “Studies in the Rural History of Upper Hesse, 1650–1830” (Ph.D. dissertation, Vanderbilt University, 1976), 18–22, 28, 32; Demandt, *Geschichte des Landes Hessen*, 288; Atwood, *The Hessians*, 160; Horst Dippel, *Germany and the American Revolution* (Chapel Hill, 1977), 238; and Gregory Wick Pedlow, “The Nobility of Hesse-Kassel: Family, Land, and Office, 1770–1870” (Ph.D. dissertation, Johns Hopkins University, 1979), 14.

²⁵ Humboldt, as quoted in Hermelink and Kaehler, *Philipps-Universität zu Marburg*, 427. For other contemporary views, see *Briefe eines reisenden Dänen* (Züllichau, 1793), 83, and *Briefe eines jungen Reisenden durch*

Both Hesse-Cassel and the American colonies had fought alongside Great Britain in its recent conflict with France. Yet, while the French and Indian War had been a mixed blessing for the American economy, the Seven Years' War had been uniformly disastrous for Hesse-Cassel. The country had served as a major battleground; many Hessian cities had changed hands four or five times. By war's end, commercial activity had fallen to less than half of its prewar level. In the countryside the depredations of both armies had consumed most of the country's crops and livestock and reduced much of the population to the verge of starvation. As late as five years after the conclusion of peace, conditions remained so bad that government officials reported that masses of Hessian poor were still emigrating to other principalities to beg for food.²⁶

Given the country's slender natural resources, small middle class, and devastated economic base, the Hessian regime—like most German governments—felt compelled to assume an active role in reviving the economy, if only because alternate sources of capital for investment did not exist.²⁷ What rendered Hesse-Cassel unique among the German states is that it looked to its military establishment as the principal source of income for funding its economic recovery. To be sure, the military had always enjoyed a high profile in Germany, principally because the empire had for centuries been used as an international recruiting ground and the German princes had regularly committed their small territorial armies to imperial or foreign service. Nor had the successes of Frederick the Great and corresponding fantasies of Joseph II done much to alter the prevalent contemporary German view of warfare as an integral part of the natural order.²⁸ In this context, Landgrave Frederick was hardly remarkable in perceiving military leadership both as part of a prince's obligations to his subjects' security and as an instrument for bringing the ruler closer to those he ruled.²⁹

In maintaining an army of great size, however, one that numbered as high as twenty-four thousand men (or one out of every fifteen inhabitants), Frederick was building on a distinctly Hessian tradition. Since the early eighteenth century the crown had come to appreciate the multiple role that a mercenary army performed in gainfully employing Hesse's excess population, artificially stimulating the country's depressed textile and iron industries, and establishing the crown's fiscal independence from the estates.³⁰ In fact, Hessian society in general valued the

Liefeland, Kurland, und Deutschland, 2 (Erlangen, 1777): 79; *Bemerkungen eines Reisenden durch Deutschland, Frankreich, England, und Holland in Briefen an seine Freunde*, 1 (Altenburg, 1775): 6; and Adolph Freiherr Knigge, *Der Roman meines Lebens*, 2, pt. 4 (Liechtenstein, 1978): 158; and Heinrich König, "Althessische Silhouetten," in *Hessisches Jahrbuch für Landesgeschichte* (Kassel, 1854), 51.

²⁶ Gary B. Nash, *The Urban Crucible: Social Change, Political Consciousness, and the Origins of the American Revolution* (Cambridge, Mass., 1979), 233–63; Hugo Brunner, *Kassel im siebenjährigen Kriege* (Kassel, 1884), 192; Walter Kürschner, *Das Werden des Landes Hessen* (Marburg, 1950), 112; memorial of November 12, 1760, StAMg, 4e) 2665; report of October 31, 1763, *ibid.*, 5) 7061; report of February 24, 1768, *ibid.*, 17 II) 23; and Irmgard Fees, "Wirtschaft und Wirtschaftsformen," in Wegner *et al.*, *Aufklärung und Klassizismus*, 24.

²⁷ Hans-Joachim Braun, "Economic Theory and Policy in Germany, 1750–1800," *Journal of European Economic History*, 4 (1975): 313.

²⁸ Gerhard Ritter, *The Sword and the Scepter: The Problem of Militarism in Germany*, 1 (Coral Gables, Fla., 1969): 22–23.

²⁹ Frederick II, "Pensées sur les princes," ¶6.

³⁰ Hans Philippi, *Landgraf Karl von Hessen-Kassel, 1654–1730* (Marburg, 1980), 6, 8, 11–12; and Karl E. Demandt, "Die hessischen Landstände nach dem 30jährigen Krieg," in Dietrich Gerhard, ed., *Ständische Vertretungen in Europa im 17. und 18. Jahrhundert* (Göttingen, 1974), 181–82.

Soldatenhandel for its role in stimulating the economy, keeping taxes low, and providing attractive career alternatives for all social groups. The landgrave himself was encouraged in his maintenance of a mercenary army both by the active support of the Hessian diet and by the urban and rural poor, who helped the army maintain a peacetime strength of about twelve thousand men, even though enlistment was voluntary and restricted to the unskilled and landless peasants. Hence, although it might appear Hesse's "traffic in human flesh conflicted resoundingly with enlightenment ideals," the practice was sustained by something approaching a national consensus and corresponded to the country's own unfortunate economic and demographic conditions.³¹

BOTH THE EXISTENCE AND POPULAR ACCEPTANCE of such a military establishment is remarkable; equally noteworthy is the degree to which the Hessian privileged classes cooperated with the Frederician regime's ambitious social programs. Recent scholarship has emphasized the existence of considerable opposition to domestic reform in most of the Old Regime's "absolute" monarchies, whether that resistance came from the church, the nobility, the government bureaucracy, or the general population. In Hesse-Cassel, however, the crown enjoyed support both from the bureaucracy and from the Hessian estates. In fact, it is fair to say that the exchange of ideas among the crown, bureaucracy, and estates was such that "enlightened absolutism" in Hesse-Cassel constituted more a cooperative effort than the work of a single individual.

Although his own administrative apparatus was closely modeled after Prussia's and was even staffed by several former Prussian civilian and military officers, the Hessian landgrave never confronted the kind of bureaucratic obstructionism that frequently frustrated Frederick the Great.³² The landgrave clearly benefited from the more manageable size of the Hessian state and administration, which placed a minimum number of intermediary levels between him and the country, thereby enabling him to attend personally to the most trivial details and local problems, and to devise solutions that could not be effectively diluted or defused by recalcitrant bureaucrats. Indeed, he often joined his ministers in deliberations over such minor problems as rowdiness at a village inn, simple adultery among peasants, or even the levy of a five florin fine on a hunter who had trespassed in state forests and shot "an old hare."³³

The advantages and privileges that the crown bestowed on its civil servants also played a role in guaranteeing their support of its policies. Although roughly half of the ministerial positions were filled by the noble-born, fully two-thirds of the top one hundred and fifty government officers were commoners.³⁴ Their predom-

³¹ Landtagsabschiede for 1764, 1772, and 1774, StAMg, 5) 14793–95; Reglement, December 6, 1782, in *Sammlung Fürstlich hessischer Landesordnungen und Ausschreiben* [hereafter, *HLO*], 6 (Kassel, 1786): 55–67; Demandt, "Die hessischen Landstände," 182; and Higginbotham, *The War of American Independence*, 131.

³² Hans Rosenberg, *Bureaucracy, Aristocracy, and Autocracy: The Prussian Experience, 1660–1815* (Cambridge, Mass., 1958), 196–201; and Hubert Johnson, *Frederick the Great and His Officials* (New Haven, 1975).

³³ Resolution of May 17, 1763, StAMg, 5) 11133; Frederick to Marburg Regierung, July 2, 1782, *ibid.*, 5) 11998; report of October 15, 1785, *ibid.*, 5) 12000; rescript of September 7, 1773, *ibid.*, 17 11) 1090; and protocol of February 1, 1774, *ibid.*

³⁴ This and succeeding data on the Hessian bureaucracy is drawn from the lists of civil servants in executive

inance was protected both by a merit system that was rigorously enforced, despite occasional pressure from aristocratic job seekers, and by a policy of steady advancement by seniority. Moreover, the landgrave fortified their social position with legal privileges normally reserved for the nobility, including a new *Rangordnung* that made status at court dependent on the level of state office held rather than on personal lineage. Under Frederick, higher bureaucrats were able to marry into noble families, a process that he occasionally facilitated by ennobling civil servants as a further reward for distinguished service.³⁵ The high percentage of commoners within the Hessian bureaucracy did not prevent it from becoming highly exclusive. Although more than a third of the senior officeholders originally came from outside the country, they were readily integrated into what constituted a closed elite, with an overwhelming majority of its members marrying into other bureaucratic families and having brothers and sons also enter the civil service. It remained, however, a group that was dependent on the crown for its advancement and preservation.

The loyalty of these civil servants should not be allowed to obscure their own commitment to reform, a characteristic frequently exhibited by the small German bureaucracies.³⁶ Certainly, the Wolffian emphasis on the active role of the bureaucracy in furthering the popular welfare prepared them for service in the Frederician regime, as did the cameralist curriculum offered at nearby Göttingen, where one-third of the senior officeholders received at least part of their education.³⁷ This commitment was also evident among the 20 percent of this group who had published on subjects that included not only constitutional law and cameral science but also important social issues such as the elimination of serfdom and religious intolerance.³⁸ A further sign of their progressive social consciousness lay in their domination of three of Cassel's four masonic lodges, which were active in their discussion of both abstract philosophical questions and tangible social problems. In fact the Hessian Assessor Adolf Baron Knigge was the driving force behind northern Germany's Illuminati movement, which was soon suppressed in the south because of its supposedly radical political and social views.³⁹ The most incontrovertible proof of the Hessian bureaucracy's progressive social outlook

positions in the *Fürstlich Hessen-Casselscher Staats- und Adresskalender* [hereafter, SAK], 1–22 (Kassel, 1764–85), and from biographical and genealogical data provided in Friedrich Wilhelm Strieder *et al.*, *Grundlage zu einer hessischen Gelehrten, Schriftsteller, und Künstler Geschichte vom 16. Jahrhundert bis auf gegenwärtigen Zeiten*, 1–21 (Kassel, 1781–1863).

³⁵ *Rangordnung*, March 13, 1762, in *HLO*, 6: 42–44; and Pedlow, "The Nobility of Hesse-Kassel," 206, 251–52.

³⁶ Most prominent among the studies that emphasize the reforming commitment of the bureaucracy in the small German states are Helen P. Liebel, *Enlightened Bureaucracy versus Enlightened Despotism in Baden, 1750–1792* (Philadelphia, 1965); Loyd E. Lee, *The Politics of Harmony: Civil Service, Liberalism, and Social Reform in Baden, 1800–1850* (Newark, Del., 1980); Bernd Wunder, *Privilegierung und Disziplinierung: Die Entstehung des Berufsbeamtentums in Bayern und Württemberg (1780–1825)* (Munich, 1978); and Hildcgard Flurschütz, *Die Verwaltung des Hochstifts Würzburg unter Franz Ludwig von Erthal (1779–1795)* (Würzburg, 1965), 106–09, 249.

³⁷ Hermelink and Kaehler, *Philipps-Universität zu Marburg*, 357–58; Charles E. McClelland, *State, Society, and University in Germany, 1700–1914* (Cambridge, 1980), 59–60; and Götz von Selle, *Die Matrikel der Georg-August-Universität zu Göttingen, 1734–1837*, 1 (Hildesheim, 1937).

³⁸ Strieder, *Grundlage zu einer hessischen Gelehrten, Schriftsteller, und Künstler Geschichte*, 7: 189–91, and vols. 1–21 *passim*; and König, "Althessische Silhouetten," 35.

³⁹ Adolph Kallweit, *Die Freimaurerei in Hessen-Kassel* (Baden-Baden, 1966), 12–14, 63–64, 341–47; Kurt

appears in internal government correspondence: individual officials frequently bemoaned the plight of the landgrave's subjects and typically expressed their eagerness to initiate their superiors' plans for reforms or new domestic programs.

The cooperation and participation of the bureaucracy in the Frederician reform program is significant, if not altogether unusual for a German civil service—the Prussian example notwithstanding. What is most unusual was the crown's close alliance with the Hessian diet. It has been commonplace for historians to characterize the Continental parliaments as both inflexibly conservative and obstructionist, because they opposed the “progressive” historical forces of centralized government and Enlightenment rationalism. Although F. L. Carsten in a landmark study took the fresh approach of defending the role of the German estates as a counterpoise to princely authority, he never disputed the record of “strenuous opposition of the Estates to the principles of absolute government.”⁴⁰ Recent studies of the individual German principalities have also confirmed the traditional view of parliamentary opposition to the crown, especially over fiscal matters.⁴¹ Nor has modern Hessian scholarship deviated from the practice of attributing the Frederician reform initiatives solely to the ruler and his ministers, doubtless because these have proven the customary source in the other states of the Old Regime. Although both Otto Berge's superb study of Frederick and Karl E. Demandt's groundbreaking work on the Hessian *Landtag* demonstrate a familiarity with the final protocols, or *Abschiede*, concluded between the crown and estates at the end of each diet,⁴² no one has ever examined either the hundreds of gravamina presented at each meeting or the extensive records of the ensuing bilateral negotiations. This is regrettable, especially since the diet usually recessed and drew up the *Abschiede* long before the crown had investigated and responded to most of its *desiderata*, electing to entrust the continuing negotiations to a standing committee of deputies.⁴³ Hence, what the *Abschiede* fail to reflect is the wealth of parliamentary initiatives that the Frederician regime eventually incorporated into its extensive domestic reform program. The lists of gravamina and records of subsequent negotiations with the crown present the unmistakable profile of a parliamentary body that worked closely with the

Kersten, *Der Weltumsegler Johann Georg Adam Forster, 1754–1794* (Bern, 1957), 93–94; Both and Vogel, *Friedrich II. von Hessen-Kassel*, 86; Epstein, *The Genesis of German Conservatism*, 87–111; and Max Spindler, *Handbuch der bayerischen Geschichte*, 2 (Munich, 1966): 1027–33.

⁴⁰ Gerhard Oestreich, “Ständestaat und Ständewesen im Werk Otto Hintzes,” in Gerhard, *Ständische Vertretungen in Europa*, 56–71; and Carsten, *Princes and Parliaments in Germany from the Fifteenth to the Eighteenth Century* (Oxford, 1959), 444.

⁴¹ Karl Otmar Freiherr von Aretin, *Bayerns Weg zum souveränen Staat: Landstände und konstitutionelle Monarchie, 1714–1818* (Munich, 1976), 23–60; Hartmut Lehmann, “Die württembergischen Landstände im 17. und 18. Jahrhundert,” in Gerhard, *Ständische Vertretungen in Europa*, 195–99; Wunder, *Privilegierung und Disziplinierung*, 33–97; Karl Bosl, *Die Geschichte der Repräsentation in Bayern* (Munich, 1974); Blanning, *Reform and Revolution in Mainz*, 126–38, 141–62; Friedrich Keinemann, *Das Domkapitel zu Münster im 18. Jahrhundert* (Münster, 1967), 169–74; Reinhard Renger, *Landesherr und Landstände im Hochstift Osnabrück in der Mitte des 18. Jahrhunderts* (Göttingen, 1968), 136–42, 145–46; and Flurschütz, *Die Verwaltung des Hochstifts Würzburg*, 97–101, 248.

⁴² Berge, “Innenpolitik des Landgrafen Friedrich II.”; and Demandt, “Die hessischen Landstände im Zeitalter des Frühabsolutismus,” *Hessisches Jahrbuch für Landesgeschichte*, 15 (1965): 38–108.

⁴³ For example, it took the crown two years to examine and respond to the gravamina of the important diet in 1764 and three years to complete negotiations with the deputies. Lennep to Riedesel, September 8, 1766, StAMg, 5) 14726; and Actum Cassel Regierung, February 26, 1767, *ibid.*

Frederician regime and virtually represented the needs of the general population, not just the enfranchised nobles and burghers who elected its deputies.

If the role of the Hessian diet was unusual, it can be attributed to the equally unique fiscal and constitutional conditions that obtained in Hesse-Cassel. For his entire reign Frederick's extensive income from the *Soldatenhandel* made him fiscally independent of the Hessian *Landtag*. As early as 1764 his treasury boasted a balance of 8.5 million taler, or the equivalent of ten years' ordinary revenue; the 12 million taler profit he turned from the American Revolution provided him with an additional thirteen years' supply.⁴⁴ As a result he made only modest fiscal demands on the estates. In 1764 he even cancelled one tax levy that they had already voted and by the following year had rescheduled another five-year appropriation over a thirty-six-year period (1766–1802). Following the conclusion of the treaty with Great Britain in 1776, which provided substantial subsidies, he halved all taxes for the duration of the reign.⁴⁵ These measures did not prevent subsequent diets from seeking still further tax reductions or from trying to share in the dispensation of government surpluses.⁴⁶ Nevertheless, the virtual absence of fiscal pressures spared the landgrave from serious confrontation with his estates and enabled him to negotiate with them in an atmosphere free of conflict or mutual suspicion. Frederick's largesse also benefited the Hessian *Ritterschaft*, which dominated one of the diet's two chambers. By the second half of his reign, its members could rely on the landgrave's treasury for low interest loans. Moreover, the Hessian *Ritterschaft* was unusually dependent on the crown for its livelihood; as many as 90 percent of its adult members sought careers in court, administrative, or military positions, both because they could not support themselves by agriculture and because the new *Rangordnung* obliged them to secure a government position in order to achieve the same high sociopolitical status enjoyed by bourgeois civil servants and army officers.⁴⁷ Even the thirty generally more wealthy and independent nobles who sat in the *Landtag* between 1764 and 1785 included twelve high government officials, in addition to two court and four military officers.⁴⁸

Crown finances and patronage exercised a unifying influence in Frederick's relations with the nobility. Equally unifying was his domestic policy. Although the landgrave was fairly faithful to Enlightenment ideas, his programs never threatened the underlying social structures of Hessian society. Furthermore, since most of Frederick's initiatives were aimed at bolstering the country's economy, he usually enjoyed the support of the impoverished Hessian towns and aristocracy represented in the diet, especially since he was essentially employing his own funds to run his new programs. In addition, many of the nobles who voted in the *Landtag*—and

⁴⁴ Josef Sauer, *Finanzgeschäfte der Landgrafen von Hessen-Kassel* (Fulda, 1930), 31–32; and Berge, "Innenpolitik des Landgrafen Friedrich II.," 31.

⁴⁵ Steuerkolleg report, November 17, 1765, StAMg, 5) 7061; Landtagsabschied, 1764, *ibid.*, 5) 14703; Steuerkolleg report, January 26, 1778, *ibid.*, 5) 3542; Adolph Lichtner, *Landesherr und Stände in Hessen-Kassel, 1797–1821* (Göttingen, 1913), 10; and Sauer, *Finanzgeschäfte der Landgrafen von Hessen-Kassel*, 28–29.

⁴⁶ Landtagsabschied, 1772, StAMg, 5) 14894; Geheimrat protocol, March 5, 1779, *ibid.*, 5) 14737; pro memoria, April 9, 1779, *ibid.*, 5) 14739; and D. B. W. Pfeiffer, *Geschichte der landständischen Verfassung in Kurhessen* (Kassel, 1834), 150.

⁴⁷ Pedlow, "The Nobility of Hesse-Kassel," 204–05, 209.

⁴⁸ Landtagsabschiede of 1764, 1773, 1779, 1786, StAMg, 5) 14793–97; and SAK, vols. 1–22.

especially those deputies who simultaneously served the administration (and whose views can therefore be traced through intragovernment memoranda)—were genuinely committed to finding new solutions to the country's unsolved economic and social problems. Their support suggests that noble opposition to "enlightened absolutism," which was so evident in Europe wherever it threatened the practical self-interest of the nobility, may have been defused in Hesse-Cassel once the nobility no longer perceived the crown as an adversary. Meanwhile, given the growing percentage of German nobles who attended universities during the eighteenth century (about 25 percent in Frederician Hesse and at least 50 percent in its diet) the greater exposure to Enlightenment ideas may have eroded the ideological base for noble opposition.⁴⁹

Indeed, one of the keys to the landgrave's close relationship with his estates was his willingness to honor their remaining constitutional privileges and sensibilities. Even though Frederick had the power to levy taxes on his own authority, he continued to let them negotiate and collect taxes, and he made a practice of consulting with them on most important matters. Given his unassailable fiscal position, Frederick could afford to be deferential. In addition, however, he and his ministers were inclined to practice absolutism within the traditional constitutional structure of a *Ständestaat*. This had been the message of Burlamaqui, a life-long advocate of the sanctity of corporate institutions and champion of Montesquieu's political thought.⁵⁰ It was also an inherent part of the cameralist philosophy being taught to future Hessian bureaucrats at nearby Göttingen, where the curriculum reflected not only the strong Anglo-Hanoverian parliamentary tradition but also that university's successful effort to lure young German noblemen as students.⁵¹

The Frederician regime's willingness to honor the sanctity of established corporate bodies demonstrates that an absolute monarchy—one in which sovereignty in all substantive matters lies with the crown—could function within a *Ständestaat*.⁵² In this regard the Hessian state also offers striking parallels and contrasts with the constitutional crisis unfolding in Great Britain's American colonies. Colonial parliamentary institutions at the outbreak of the American Revolution were not wholly dissimilar to their Hessian counterparts, both of which stemmed from the idealized notion of a tripartite division of power between a royal executive and a bicameral legislature split between an aristocratic and popular electorate.⁵³ Although the franchise was more restricted in Hesse-Cassel, the diet's upper chamber (secularized benefices and the nobility) paralleled the colonial upper houses and

⁴⁹ Karl Wilhelm Justi, *Grundzüge einer Geschichte der Universität zu Marburg* (Marburg, 1827), 104; Pedlow, "The Nobility of Hesse-Kassel," 22; Selle, *Matrikel der Georg-August-Universität*; and Max Eberhard Habicht, *Suchbuch für die Marburger Universitäts-Matrikel von 1653–1830* (Darmstadt, 1927).

⁵⁰ Cobban, *In Search of Humanity*, 161–62; and Peter Gay, *Voltaire's Politics: The Poet as Realist* (New York, 1965), 195.

⁵¹ Carol Rose Loss, "Status in Statu: The Concept of Estate in the Organization of German Political Life, 1750–1825" (Ph.D. dissertation, Cornell University, 1970); Rudolf Vierhaus, "Die Landstände in Nordwestdeutschland im späten 18. Jahrhundert," in Gerhard, *Ständische Vertretungen in Europa*, 78–80, 92–93; H. Christern, *Deutscher Ständestaat und englischer Parlamentarismus am Ende des 18. Jahrhunderts* (Munich, 1939), 105–207; and McClelland, *State, Society, and University in Germany*, 43–46.

⁵² On this point, see Robert M. Berdahl, "The *Stände* and the Origins of Conservatism in Prussia," *Eighteenth-Century Studies*, 6 (1973): 300.

⁵³ Gordon S. Wood, *Representation in the American Revolution* (Charlottesville, 1969), 25.

even the Continental Congress in their domination by old and well-established landholding families; similarly, the impoverished Hessian burghers who dominated the *Landtag's* lower chamber were no more wealthy (and frequently considerably poorer) than their American counterparts in the colonial lower houses, especially where restrictive voting qualifications and the habitual electoral domination by certain patrician families were normal. Even the American practice of ensuring electoral responsibility through binding instructions was common to Hesse-Cassel, which traced the practice of drawing up instructions for its deputies at local assemblages to the same late medieval roots.⁵⁴

The social structure of the two societies, of course, was not the same. Yet the conflict in America did not arise primarily over social issues. Although there was some movement in both countries to correct the worst social inequities, whether serfdom in Hesse or slavery in America, legislative institutions and the people who maintained them remained socially conservative in their acceptance of an essentially hierarchical society.⁵⁵ Rather, what distinguished the two societies—and what principally brought on America's break with Great Britain—were constitutional issues regulating the relations between the crown and its subjects, issues that had already been resolved in Hesse-Cassel. Whereas Hesse-Cassel's lucrative military system permitted the landgrave to lower taxes, pursue popular economic programs, and defer to existing parliamentary institutions, Great Britain's urgent need to fund its own past and present military commitments drove it in the opposite direction of increased taxation, unpopular mercantile policies, and the circumvention of the colonies' established legislative bodies.

BECAUSE OF THE WIDESPREAD POVERTY IN HESSE-CASSEL, the great bulk of governmental initiatives not surprisingly dealt with the economy. Frederick's extensive financial resources allowed him to support a comprehensive program of economic development that embraced all of the standard eighteenth-century strategies of boosting investment, maximizing the use of available land, labor, and technology, and developing new products and markets.⁵⁶ In a significant shift from earlier mercantilist practice, government policy reflected the considerable influence of physiocratic ideas on contemporary German cameralist thought by emphasizing the

⁵⁴ Jackson Turner Main, *The Upper House in Revolutionary America, 1763–1788* (Madison, Wis., 1967), 188–89; Richard Brown, "The Founding Fathers of 1776 and 1787: A Collective View," *William and Mary Quarterly*, 33 (1976): 466–67; Jon Teaford, *The Municipal Revolution in America: Origins of Modern Urban Government, 1650–1825* (Chicago, 1975), 56–61, 65; Wood, *Representation in the American Revolution*, 40–45; and Bailyn, *The Ideological Origins of the American Revolution*, 161–66.

⁵⁵ Morgan, *American Slavery, American Freedom*, 6, 385; Bailyn, *The Ideological Origins of the American Revolution*, 302–03; Wood, *Representation in the American Revolution*, 6–12; and Robert R. Palmer, *The Age of the Democratic Revolution: A Political History of Europe and America, 1760–1800*, volume 1: *The Challenge* (Princeton, 1959), 187–213.

⁵⁶ The most comprehensive treatments of Frederician economic policy are found in Berge, "Innenpolitik des Landgrafen Friedrich II.," 80–170; and Both and Vogel, *Friedrich II. von Hessen-Kassel*, 40–58. Also see Fees, "Wirtschaft und Wirtschaftsreformen," 23–33; Eduard Brauns, "Die Agrarpolitik Landgraf Friedrichs von Hessen," *Hessische Heimat*, 17 (1967): 82–86; Otfried Dascher, *Das Textilgewerbe in Hessen-Kassel vom 16. bis 19. Jahrhundert* (Marburg, 1968), 54–165 *passim*, and "Die hessische Handlungskompagnie zu Karlshafen (1771–1789)," *Hessisches Jahrbuch für Landesgeschichte*, 22 (1972): 229–53; and Gottfried Ganssauge, "Bauernsiedlungen des Landgrafen Friedrich II.," *Hessische Heimat*, 5 (1939): 17–24.

primacy of the agricultural sector in the overall economy.⁵⁷ Although Frederick and his ministers drafted most economic policy, frequently by borrowing from the successes of other states, the Hessian estates invariably supported the regime, not only encouraging those urban initiatives in which they had the greatest stake but even advocating government subsidies for new factories and commercial enterprises.⁵⁸ Perhaps the cooperation between the estates, crown and bureaucracy was best exemplified by the creation and activity of the *Gesellschaft für Ackerbau und Kunst*, a private society that undertook to encourage economic development by promoting the discussion of economic problems and the dissemination of new methods and technology. Although Frederick had advocated such a society as early as 1760, it owed its creation five years later to the lobbying of a prominent member of the diet, who also served for a time as its president. Throughout its operations its membership always comprised a mix of high government officials and *Landtag* deputies.⁵⁹

The Hessian estates, though most directly affected by the regime's economic programs, also became involved in virtually all other areas of government activity, even administrative matters. As early as 1764 they had complained about the chaotic state of the bureaucracy, singling out not only its backlog of unattended cases but also the absence of any organized register of laws that could be consulted by officials and laymen alike.⁶⁰ In fact, Frederick also spent much of the next decade trying to streamline the bureaucracy by restructuring it along Prussian lines, even to the point of renaming individual departments to correspond with their Prussian counterparts.⁶¹ Given the contemporary physiocratic and cameralist emphasis on rationally organized and efficient government, it is unlikely that the regime was motivated solely by the remonstrances of the estates. Their hand is clearly evident, however, in the *Sammlung Fürstlich Hessischer Landesordnungen*, the country's first compendium of all laws and decrees enacted since the Middle Ages, which was published three years after the diet's initial appeal. They also provided the impetus in the creation of Hesse-Cassel's provincial governors, or *Landräte*, in 1773. The *Landräte* eventually became the Hessian regime's eyes and ears at the local level and proved invaluable both in implementing its programs and in

⁵⁷ Agriculture was "the only true source of the country's wealth," as the ministry put it in one report to the landgrave; Actum Regierung Cassel, March 30, 1786, StAMg, 5) 14746. Also see Ulrich Fr. Kopp, "Von den Landräten," in von Berg, ed., *Hessen-Casselisches Teutsches Staatsmagazin* (1796), 110–11; Lothar Zögner, *Hugenottendörfer in Nordhessen* (Marburg, 1966), 41; and John G. Gagliardo, *From Pariah to Patriot: The Changing Image of the German Peasant, 1770–1840* (Lexington, Ky., 1969), 34–38, 40.

⁵⁸ Resolution of March 1, 1763, StAMg, 5) 5094; "Desideria Generalia . . . 3^{tes}," *ibid.*, 5) 14717; Geheimrat protocol, July 4, 1764, *ibid.*, 5) 14715; Landtagsabschied, 1764, *ibid.*, 5) 14793; pro memoria, December 11, 1773, *ibid.*, 5) 5528; and Georg Riedesel, *Eine Rede bey der Ehrensäule Friedrichs des Zweiten* (Kassel, 1783), 4, 7.

⁵⁹ Frederick II, "Pensées sur les princes," ¶5; Capellan to Frederick, October 5, 1765, StAMg, 4a) 91, 17; W. Gerland, "Die Tätigkeit der . . . Gesellschaft für Ackerbau und Kunst," *Landwirtschaftliche Jahrbücher*, 59 (1923–24): 245–89; and "Aus den Tagen der althessischen Gesellschaft des Ackerbaues," *Hessenland*, 25 (1911): 171–73, 193–95.

⁶⁰ "Desideria Communia in Justiz Sachen," StAMg, 5) 14717; and Landtags Commissarius report, August 23, 1764, *ibid.*, 5) 14715.

⁶¹ Protocol of May 5, 1773, StAMg, 5) 5399; resolution of May 11, 1773, *ibid.*; Felix Rosenfeld, "Geheime Kanzleien und Kabinett in Hessen-Kassel," *ZhG*, 51 (1917): 137; and Friedrich Israel, "Die Kriegs- und Domänenkammer Landgraf Friedrichs II. und ihre Wurzeln," *Mitteilungen an die Mitglieder des Vereins für hessische Geschichte und Landeskunde* (1925–26), 90.

assessing their effects. Yet their establishment stemmed from the *Landtag* of 1772, which successfully sought such local officials in order to supervise the administration of tax revenue in the provinces.⁶² This appeal for greater crown oversight in tax collection constitutes an aberration to the early modern historian, who is accustomed to recording the estates' tenacious opposition to royal fiscal control at the local level. This incongruity has a corollary, however, in the diet of 1764, which urged Frederick to complete a new land and tax register in order to eliminate the inequities inherent in Hesse's existing, century-old cadaster.⁶³ The documents themselves do not point to any hidden, less altruistic motives behind the diet's remonstrances. Thus, we can only conjecture whether its stance in favor of greater administrative efficiency stemmed from the unusually relaxed fiscal environment caused by the *Soldatenhandel* or perhaps by the presence of a large number of high government officials in the *Landtag* itself. What remains clear, however, is the diet's cooperation with the regime's attempts to establish a more rational administrative structure.

If the estates assisted Frederick in streamlining the bureaucracy, they also helped lay the foundation for his legal reforms with a series of petitions, including a lengthy, twenty-two-point blueprint designed to improve the quality, equity, and efficiency of the Hessian courts. In taking a series of steps aimed at eliminating the years-long backlog of lawsuits and criminal cases, the landgrave was merely acting on an earlier pledge to the estates.⁶⁴ His efforts to bolster professionalism within the judiciary by increasing judges' salaries and depriving the bureaucracy of its own judicial powers also stemmed directly from the appeals of the diet. Even the landgrave's decision to give free legal aid and advice to peasant litigants had its genesis in the diet of 1764 to which he had promised relief from the high fees of country lawyers.⁶⁵

Although the *Landtag* helped direct the thrust of Frederick's reform of the Hessian legal system, its influence should not obscure his own contribution. The diet advocated essentially tactical measures that were designed to resolve widely perceived practical problems; the landgrave superimposed the theoretical constructs of Beccarian jurisprudence on the existing legal code. In addition to eliminating torture and excessively harsh sentences for minor offenses, he insisted on removing even the most trivial forms of corporal punishment.⁶⁶ He was also sensitive to the contemporary utilitarian emphasis on the paramount importance of

⁶² Pro memoria, August 22, 1772, StAMg, 5) 14794; and concordat of September 22, 1772, *ibid.*

⁶³ Desiderium, Nr. 30, StAMg, 5) 14715; Geheimrat protocol, July 4, 1764, *ibid.*; and Steuerkolleg report, July 14, 1764, *ibid.*

⁶⁴ For the diet's remonstrances and the crown's pledge of reform, see "Desideria Communia in Justiz Sachen, 19^{tes}," StAMg, 5) 14717; "Protocol von Land-Tags Sachen, 19," *ibid.*, 5) 14722; and Actum Regierung Cassel, February 26, 1767, *ibid.*, 5) 14726. For the reforms themselves, see rescripts of June 21, July 6, 1770; April 13, 1773; August 23, October 25, 1774; and February 28, 1775, in *HLO*, 574, 578–86, 685–86, 775–76, 785–86, 814–16; and L. H. L. G. von Canngiesser, Introduction to *Collectionis notabiliorum Decisionum supremi tribunalis appellationum Hasso-Cassellani*, 1 (Kassel, 1768).

⁶⁵ "Desideria Communia in Justiz Sachen, 4^{tes}, 6^{tes}, 11^{tes}," StAMg, 5) 14717; rescripts of August 23, October 25, 1774, in *HLO*, 775–76, 785–86; rescript of February 10, 1775, *ibid.*, 807–11; and Geheimrat protocol, June 9, 1767, StAMg, 5) 11116.

⁶⁶ Berge, "Innenpolitik des Landgrafen Friedrich II.," 172–76. In requiring an end to corporal punishment, the landgrave occasionally crossed swords with his ministers and the members of the *Landtag*, who continued to

crime prevention. Hence he made a point of announcing all criminal convictions in the widely read *Polizei-Zeitung* and simultaneously directed his ministers to suppress all news of the discontinuation of corporal punishment, lest its elimination serve as an “*invitatio ad delinquendum*.”⁶⁷ Perhaps more significant was the regime’s perception of poverty as a major cause of crime. This realization led it to reduce even further fines and prison terms in cases where such punishment threatened to destroy an individual’s livelihood and also inspired attempts to convert existing prisons and poorhouses into workhouses where inmates could learn a useful trade and strengthen their work ethic.⁶⁸ Indeed, an underlying motivation behind Frederick’s establishment of Germany’s first state-run homes for unwed mothers and foundlings was that their creation would dissuade poor or wayward mothers from committing infanticide—a crime that had assumed epidemic proportions following the Seven Years’ War.⁶⁹

In addition to forestalling serious crime, the *Accouchir- und Findelhaus* was also part of a comprehensive government program aimed at reducing poverty. During Frederick’s reign a state-run system of poor relief replaced the increasingly overtaxed private and municipal institutions that had relied on voluntary contributions.⁷⁰ At the urging of the diet, this relief was supplemented by a *Witwenkasse* specifically designed to assist indigent widows.⁷¹ In addition, however, the regime went beyond remedial efforts to alleviate poverty to prophylactic measures aimed at its prevention. Given the crown’s strong fiscal position, the landgrave frequently granted tax relief to those peasants in dire need. Taxes were routinely forgiven following natural disasters, just as they had been after the losses of the Seven Years’ War, and were postponed indefinitely whenever they would force a peasant to pawn or sell his tools, livestock, or other means of production. As a further aid to hard-pressed farmers, the government also made it more difficult for noble overlords to buy out small freeholders and virtually impossible for creditors to foreclose in case of default.⁷² Meanwhile, as a last resort there remained mass state employment: the army for peasants and the huge construction gangs of urban poor

feel that some form of physical punishment had a greater deterrent effect than fines and imprisonment. Actum Cassel Regierung, September 10, 1776, and rescript, September 20, 1776, StAMg, 5) 11138; and “Protocol von Land-Tags Sachen, 16.,” *ibid.*, 5) 14722.

⁶⁷ Both and Vogel, *Friedrich II. von Hessen-Kassel*, 40; and Berge, “Innenpolitik des Landgrafen Friedrich II.,” 170, 180.

⁶⁸ Verordnung, March 17, 1767, in *HLO*, 412; “Gantz gefällige Gedancken . . .,” n.d., StAMg, 5) 13461; Geheimrat protocol, March 12, 1782, *ibid.*, 5) 11998; and Martin Eckel, “Das Kasseler Werkhaus, 1782–1823,” *ZhG*, 75–76 (1975–76): 431–40.

⁶⁹ Otto Berge, “Wohlfahrtspflege und Medizinalwesen unter Landgraf Friedrich II.,” *ZhG*, 55–56 (1954–55): 137–39.

⁷⁰ Verordnung, August 6, 1773, in *HLO*, 707–10; Eckel, “Das Kasseler Werkhaus, 1782–1823,” 431–40; and Berge, “Wohlfahrtspflege und Medizinalwesen unter Landgraf Friedrich II.,” 125–34. The government’s decision to assume control of poor relief stemmed from the realization that the level of charitable contributions was falling and was therefore inadequate to meet the rising numbers of indigent. Report of July 29, 1773, StAMg, 5) 10852; report of December 20, 1773, *ibid.*; report of November 10, 1773, *ibid.*, 5) 10853.

⁷¹ Actum Cassel Regierung, November 15, 1764, StAMg, 5) 14715. F. Münscher has attributed this act solely to Frederick; see Münscher, *Geschichte von Hessen* (Marburg, 1894), 426.

⁷² Steuerkolleg report, November 17, 1764, StAMg, 5) 7061; Steuerkolleg report, March 20, 1772, *ibid.*, 5) 3542; Landrat instructions, ¶12, *ibid.*, 5) 4294; Actum Regierung Cassel, May 21, 1778, *ibid.*, 5) 3542; Verordnung, March 18, 1774, in *HLO*, 759–60; and decrees of May 3, 1776, and April 14, 1778, *ibid.*, 863–66, 915–17.

who were employed after 1767 in the landgrave's ambitious public building program.⁷³

As significant as these efforts were, the Hessian diet was responsible for the two most dramatic measures aimed at controlling poverty by preventive means. The creation of a state-administered fire insurance fund in 1767 was clearly one of the regime's most popular initiatives, coming as it did after three years of pressure from the diet, which assured Frederick that "half the country wants the establishment of a *Brandkasse*" to protect small property owners from potentially catastrophic losses. By 1768 over half of Hesse-Cassel's almost sixty thousand buildings had been insured with the fund, whose success ultimately inspired other government initiative in the field of fire prevention—including the establishment of building codes, the acquisition of modern fire fighting equipment, and even the installation of Germany's first lightning rod by a University of Rinteln scientist.⁷⁴ At the same time the diet was responsible for a second prophylactic measure in the fight against poverty, a measure that was unquestionably the most controversial and unpopular initiatives of Frederick's reign: the so-called *Hufen Edikt* of 1773, which sought to preempt the progressive impoverishment of peasant families by prohibiting the suicidal subdivision of farmland into unprofitably small plots.⁷⁵

Finally, the estates and regime were generally of one mind in their determination to improve the quality of the Hessian education system. Frederick's reorganization of the primary schools concentrated on improving their ability to teach basic and vocational skills, a strategy that reflected the pedagogical elitism of most Enlightenment thinkers and educators.⁷⁶ He also answered their remonstrances for emergency funding to bolster the size and quality of Marburg's university faculty and library as well as increase its shrinking student body. Moreover, in the curriculum reform that he instituted in 1782, he sought to protect the Hessian universities against antirationalist influences by specifically forbidding the hiring or retention of faculty who rejected the "mathematical, pure philosophical, and historical sciences that have lifted us out of barbarism, that we have our current Enlightenment to thank for, and that are so important to educate heart and soul our young people in."⁷⁷ In the field of higher education, however, Frederick devoted most of his

⁷³ As early as 1760, Frederick extolled the importance of public works projects as a source of employment for the urban poor. See his "Pensées sur les princes," ¶26.

⁷⁴ "Desideria Generalia . . . 16^{tes}," StAMg, 5) 14717; Actum Regierung Cassel, November 5, 1764, *ibid.*, 5) 14715; Boyneburg and Scholley pro memoria, January 15, 1767, *ibid.*, 5) 10695; Otto Berge, "Wohltätige Einrichtungen," in Wegner et al., *Aufklärung und Klassizismus*, 36. To date, the creation of the fire insurance fund is the only Frederician program in which the diet's contribution has been recognized.

⁷⁵ "Desideria Generalia . . . 19^{tes}," StAMg, 5) 14717; report of April 20, 1765, *ibid.*, 17 II) 1434; and Actum Cassel Regierung, July 19, 1764, *ibid.*, 17 II) 1402.

⁷⁶ Harvey Chisick, *The Limits of Reform in the Enlightenment: Attitudes toward the Education of the Lower Class in Eighteenth-Century France* (Princeton, 1981), 278; Harry C. Payne, *The Philosophes and the People* (New Haven, 1976), 94–116; and Andreas Flitner, *Die politische Erziehung in Deutschland: Geschichte und Probleme, 1750–1880* (Tübingen, 1957), 20–21. For the diet's petitions and Frederick's policy toward primary education, see "Desideria Generalia . . .," StAMg, 5) 14717; "Desideria Generalia . . .," *ibid.*, 5) 2920; Actum Cassel Consistory, May 5, 1774, *ibid.*, 5) 2846; von W——, "Vom höheren Unterrichtswesen in Hessen unter Landgraf Friedrich II. (1760–1785)," *Hessenland*, 13 (1899): 202; and Berge, "Innenpolitik des Landgrafen Friedrich II.," 239, 243.

⁷⁷ Petition, May 8, 1764, StAMg, 304) 573; Hermclink and Kaehler, *Philipps-Universität zu Marburg*, 388–90, 392–97, 402–06, 412, 415, 420; and Dersch, "Geschichte der Universität Marburg im Zeitalter der Aufklärung," 165, 174.

attention and resources to developing Cassel's own *Collegium Carolinum*. The *Collegium* was actually only an academy that prepared the children of Cassel's elite families for the university. Yet the landgrave more than doubled its faculty by hiring such noted figures as the naturalist Georg Forster, the Swiss historian Johannes von Müller, the physiocrat Jakob Mauvillon, the cameralist Christoph Wilhelm von Dohm, and the writer Rudolf Erich Raspe. He also indulged their requests for a number of literary and political journals, as well as for several research laboratories that ranged from a menagerie and botanical garden to Germany's first anatomical theater and a colony of African blacks for the *Collegium's* noted anatomist Samuel Thomas Sömmerring.⁷⁸ The landgrave expressed the hope that the *Collegium's* upgraded faculty and facilities would enhance the cosmopolitanism of Cassel's court and bureaucracy, in addition to improving the quality of instruction at Hesse's military academy. No doubt, the landgrave also intended these luminaries to add to his international reputation as a patron of the arts and sciences, a goal that he further served by establishing a state theater, opera, museum, painting school, academy of science and fine arts, and a *Société des Antiquités*. In addition, however, the *Collegium's* science faculty performed the more practical function of spearheading the government's reform and administration of Hesse-Cassel's health care system. As early as 1767 a *Collegium Medicum* set strict professional standards for doctors, pharmacists, and midwives, closely monitored the outbreak of new diseases, and administered the training of midwives at the *Accouchirhaus*; later on it also ran Cassel's new, large *Charité* hospital for the indigent.⁷⁹

If there was one traditional area of Enlightenment reform in which the Hessian diet was not active it was religious toleration. The relatively homogenous populations of Lutheran Upper Hesse and Calvinist Lower Hesse provided little occasion for religious antagonism or controversy. Moreover, despite his vocal commitment to toleration, Frederick's conversion to Catholicism in 1749 made it difficult for him to alter the principality's restrictions on religious freedom, especially where they might have a bearing on the country's tiny Roman Catholic community. Significantly, however, he did intervene upon occasion in disputes between Lutherans and Calvinists, always with the intention of lowering religious barriers within the Hessian bureaucracy, guilds, and educational system.⁸⁰

THE BENEVOLENT NATURE OF THE FREDERICIAN REGIME and the active cooperation of the Hessian diet provide a refreshing contrast both to the current American view of the landgrave and to the standard modern interpretations of legislative institutions during the Old Regime. They do little, however, to erase our perception of the

⁷⁸ H. Schelenz, "Über das Kasseler Collegium Carolinum," *Hessenland*, 18 (1904): 79; J. H. Merck, "Über einige Merkwürdigkeiten von Cassel," *Der Teutsche Merkur* (1780), 217–18; Käthe Heinemann, "Aus der Blütezeit der Medizin am Collegium illustre Carolinum zu Cassel," *ZhG*, 71 (1960): 89–90; and König, "Althessische Silhouetten," 46.

⁷⁹ Edict, December 21, 1767, in *HLO*, 469–93; Hans-Joachim Braun, "Hessische Medizinalverhältnisse im 18. Jahrhundert," *Hessenland*, 17 (1903): 103; and Alfons Fischer, *Geschichte des deutschen Gesundheitswesens*, 2 (Berlin, 1933): 135–36.

⁸⁰ Resolution of October 24, 1760, *StAMg*, 5) 5173; Peter Hertner and Thomas Fox, "Lebensmittelpreise in Marburg, 1764–1830," in A. Imhof, ed., *Historische Demographie als Sozialgeschichte*, 2 (Marburg, 1975): 860; and

considerable circumstantial and structural differences between Old and New World societies. Although American state and municipal governments occasionally helped local industries and provided basic assistance for the disadvantaged, the new federal authority never confronted the degree of economic stagnation and widespread poverty that mandated wholesale mercantile intervention by the Hessian regime.⁸¹ In any event America had no equivalent to the cameralist ethos and extensive local administrative apparatus of the Hessian bureaucracy that helped carry out these and other governmental programs. Almost as disparate were the religious realities that divided the two societies. Although both were overwhelmingly Protestant, Hesse-Cassel had minimized confessional strife long before by enforcing uniformity, while American society was compelled by the accelerated intermingling of sectarian minorities to accept the need for toleration. To comprehend these differences is to understand the separate role that Enlightenment ideas played in America and in Hesse. In evaluating the evolution of an American political and social philosophy, it is difficult to dispute Bernard Bailyn's judgment that the realities of New World society predetermined the eventual emergence and triumph of such Enlightenment ideals as free enterprise, religious toleration, and the overall emphasis on individual freedom;⁸² in a country like Hesse-Cassel, however, where prevailing circumstances and structures were so inimical to the development of these ideals, the government simply superimposed theoretical Enlightenment constructs in an effort to overcome existing injustices.

What is so unfortunate about the Frederician reform program is that these Old World realities were impossible to overcome. Both the testimony of contemporary observers and quantitative evidence confirm that the Hessian economy recovered substantially from the losses of the Seven Years' War. Nevertheless, at the end of the reign commerce was still "of no special significance" while industry remained heavily dependent on the artificial stimulation provided by government subsidies and purchases for the army in America.⁸³ Moreover, although the regime's urbarial initiatives were instrumental in boosting agricultural productivity, this increased output merely helped sustain a 50 percent population increase that consumed these gains and exacerbated the country's chronic land shortage. At the end of the reign both the bureaucracy and diet were obliged to admit that Hesse-Cassel was still in the grips of poverty.⁸⁴ To a certain extent the government's efforts to stimulate the

Berge, "Innenpolitik der Landgrafen Friedrich II.," 241, 255. For Frederick's conversion and its effects, see Theodor Hartwig, *Der Übertritt des Erbprinzen Friedrich von Hessen-Kassel zum Katholizismus* (Kassel, 1870).

⁸¹ Merrill Jensen, *The New Nation: A History of the United States during the Confederation, 1781-1789* (New York, 1950), 283-301; and Teaford, *The Municipal Revolution in America*, 47-54.

⁸² Bernard Bailyn, "Political Experience and Enlightenment Ideas in Eighteenth-Century America," *AHR*, 67 (1961-62): 339-51.

⁸³ "Desiderium speciale," December 10, 1785, StAMg, 5) 14744; report of March 5, 1770, *ibid.*, 5) 1490; petition of March 5, 1773, *ibid.*, 5) 4015; Kriegs- und Domänenkammer report, n.d., *ibid.*; report of September 13, 1773, *ibid.*, 5) 10485; Menschen- und Viehverzeichnis, *ibid.*, 5) 10597; pro memoria, December 11, 1773, *ibid.*, 5) 5528; Johann Matthias Hassencamp, *Briefe eines Reisenden von Pyrmont . . .* (Frankfurt, 1783), 58; Brunner, *Kassel im siebenjährigen Kriege*, 290; Both and Vogel, *Friedrich II. von Hessen-Kassel*, 45-46; and Fox, "The Rural History of Upper Hesse," 28.

⁸⁴ Motz report, January 15, 1784, StAMg, 5) 13446; "Desiderium commune XXXII^{dum}" (1786), *ibid.*, 5) 14746; and W. J. C. G. Casparson, *Abhandlung von der Verhütung des Bettebels in einer Haupt- und Residenzstadt* (Kassel, 1783). Casparson, a high official in Frederick's government at the time the building program was completed, actually claimed that there were then more beggars in Cassel than at the close of the Seven Years' War.

economy and remove social inequities were limited by its own reluctance to employ more radical solutions. Thus, although it could do nothing to remove the hundreds of customs barriers that paralyzed commerce within Germany, it was also unwilling to incur the wrath of Hesse's powerful guilds by eliminating their privileges. Meanwhile, even though the regime solicited the assistance of both the Gesellschaft für Ackerbau und Kunst and its own officials in finding ways to commute feudal dues and services, their implementation was either delayed by the need to launch pilot projects or dropped altogether because it was unable to find a solution that was free of undesirable side effects for the landlord, the peasant, or the government itself.⁸⁵

When it did take action, the regime frequently discovered that even the most innocuous reforms could have unexpected repercussions and actually aggravate the problem that they were designed to correct. Hence, Frederick's thorough prussianization of the Hessian administrative offices increased its operating costs while decreasing its efficiency, largely because the more complex Prussian system was inappropriate for such a small state.⁸⁶ The new cadaster also turned into something of an embarrassment for the regime. It had been pressed by the diet and welcomed by the crown as a means of redistributing the tax burden more equitably, but both soon discovered to their horror that the new rates would substantially increase the assessments of far more peasants than originally anticipated—with the result that many local nobles and communities hindered its completion.⁸⁷ The landgrave's urbarial initiatives also included some unfortunate missteps. His attempt to prevent creditors from foreclosing on delinquent peasants had the unintended effect of driving investors out of agriculture, thereby making it more difficult for peasants to obtain credit for purchasing or improving land. The *Hufen Edikt* likewise helped scare away investors because its ban on the subdivision of small plots drove down land values inasmuch as many farmers were unwilling to purchase land that could not later be divided up among their surviving children.⁸⁸ Of course, the government's objective was to thin out Hesse's dense rural population by forcing younger children into more promising careers while enabling elder offspring to make a decent living from agriculture. For this reason it stood firm against the avalanche of petitions and protests that poured in from

⁸⁵ In its attempt to commute both the feudal tithe and service obligation, the government started out with the naive assumption that "a solution can be achieved whereby the landlord loses nothing and the peasant benefits." But it had to admit that the tithe collection could not be eliminated or modified without creating more difficulties than such a change solved. The landgrave did, however, eliminate some types of feudal service and launch a pilot program for commuting the remaining forms, a program that was still underway when Frederick died. Unfortunately, there is as yet no published account of the Frederician attempts at urbarial reform. See StAMg, 5) 3064, 5) 8723, 5) 8831; and *ibid.*, 40b) 7 Generalia 43, 40b) 19 Generalia 33, and 40b) Generalia 37.

⁸⁶ Hans Philippi and Fritz Wolff, "Staat und Verwaltung," in Wegner *et al.*, *Aufklärung und Klassizismus*, 17–18; and "Desideria ulteriora . . . 7," StAMg, 5) 14743. The most notable failure was the rural tax farm, the introduction of which resulted in lower and more irregular tax revenue and the widespread exploitation of the peasantry—exactly the opposite of the intended results. See "Ohnmassgebliche Gedancken über die Verpachtungen . . ." (1784), StAMg, 5) 13462.

⁸⁷ Pro memoria, March 11, 1772, StAMg, 5) 14721; report of January 26, 1778, *ibid.*, 5) 3542; and Actum Cassel Regierung, May 21, 1778, *ibid.*

⁸⁸ Actum Cassel Regierung, August 17, 1778, StAMg, 5) 3542; and report of December 1784, *ibid.*, 5) 3242.

Hesse's farming communities. But disinherited children were usually unable to find alternative private employment in a country whose commercial-industrial establishment was as limited as its arable land. By 1779 the estates felt compelled to seek the repeal of the law that they themselves had inspired, explaining to the government that it had merely driven disinherited offspring into beggary, emigration to other parts of Germany, or the army—and eventual resettlement in America.⁸⁹

THE THOUSANDS OF POOR PEASANT YOUTHS who enlisted in the army and subsequently stayed in America were not fleeing an oppressive government; rather, they were escaping the inflexible realities of Old World society that at least on this occasion had been aggravated by their government's well-intentioned but misconceived attempts to handle. They were not leaving in search of social equality, nor were they seeking greater freedom than was afforded them by the highly regulated and disciplined Hessian *Polizeistaat*. Indeed, although many expressed fear of the initial Atlantic crossing and of the unknown perils of the American wilderness, they generally fought well—better in fact than the other German contingents and more successfully than the mercenary forces that faced France's citizen armies two decades later. At the very least, those officers whose diaries and correspondence have survived were uniformly critical both of the colonists' refusal to pay taxes despite their obvious wealth and of their apparent hypocrisy in demanding freedom while holding black slaves in bondage.⁹⁰ What enticed many to stay in America were the economic realities of life back in Hesse-Cassel and the generous inducements given them by the Continental Congress.

The Hessian army's performance in the War of Independence does little to contradict the judgment of Friedrich Karl von Moser—himself the scourge of several less benevolent German princes—who observed "that the landgrave of Hesse-Cassel rules his land rather well, loves his subjects, and is loved by them."⁹¹ As Europe moved deeper into the Revolutionary era, there was as yet no sign that the Frederician regime—like so many other reforming governments—had incurred either the hostility of the country's privileged elements or unmanageable rising expectations and unrest among the general population.⁹² Yet, no matter how

⁸⁹ "Desideria ulteriora . . . 5," StAMg, 5) 14743 and Geheimrat protocol, March 19, 1779, *ibid.* Surviving petitions from the *Hufen Edikt* fill five thick fascicles at the Staatsarchiv in Marburg; see StAMg, 5) 13283, 17 II) 2405–08. As early as 1776, it was apparent to the regime that many new recruits were enlisting in order to emigrate to America; Ernst Kipping, *Die Truppen von Hessen-Kassel im amerikanischen Unabhängigkeitskrieg, 1776–1783* (Darmstadt, 1965), 43.

⁹⁰ Atwood, *The Hessians*, 159–60, 165–66, 170; and Kipping, *The Hessian View of America*, 25–26, 32–36. Cases of desertion among the Hessian forces both prior to and after embarkation for America usually came from recruits from other German states who had enlisted or been impressed after the outbreak of hostilities. Losch, *Soldatenhandel*, 35–36; Kipping, *Die Truppen von Hessen-Kassel im amerikanischen Unabhängigkeitskrieg*, 39–40, 43, and *The Hessian View of America*, 9–11, 13, 36; Atwood, *The Hessians*, 186–92, 204–05; and H. S. King, *Echoes of the American Revolution in German Literature* (Berkeley and Los Angeles, 1929), 172.

⁹¹ Report of September 30, 1766, Haus-, Hof-, und Staatsarchiv, Vienna, Kleinere Reichsstände 163.

⁹² Indeed, the almost total absence of popular agitation in Hesse-Cassel seems to contradict Karl H. Wegert's provocative suggestion that the German princes generally initiated reforms in reaction to popular pressure or unrest. The landgrave's programs were drawn from a variety of sources, among them the successful projects launched by other states and his own attempts to translate Enlightenment principles into social legislation. It is true that many of the regime's initiatives sprang from reports of hardships and injustices reported by *Landtag* deputies or local bureaucrats. At no time, however, were the privileged estates or the crown even remotely

much it could alleviate economic and social ills, enlightened absolutism in Hesse-Cassel could not resolve the fundamental problems of Old World society, despite the good intentions of a conscientious ruler, enthusiastic bureaucracy, and active representative body, despite the infusion of Enlightenment ideas and a Protestant social consciousness that paralleled contemporary American currents, and even despite a lucrative mercenary trade that helped reduce taxes and fund a broad range of social and economic programs. The government did, however, dispatch nineteen thousand of its subjects to the New World—and for those who stayed there, that was enough.⁹³

motivated by the kind of concern for popular unrest that Wegert has imputed to other German regimes. See Wegert, "Patrimonial Rule, Popular Self-Interest, and Jacobinism in Germany, 1763–1800," *Journal of Modern History*, 53 (1981): 440–55.

⁹³ The latest figures, compiled by Atwood, reveal that Frederick II sent 18,970 soldiers to America, of whom 3,306 settled in the New World; *The Hessians*, 256. Among those who stayed, however, were recruits from other German states who were serving in Hessian units; these non-Hessians are believed to have remained behind in disproportionately large numbers. With the exception of a handful of officers, virtually all of the native Hessians who settled in America came from the urban proletariat or the landless peasantry.

The Courts, the Magistrature, and Promotions in Third Republic France, 1871–1914

BENJAMIN F. MARTIN

ODILON BARROT, liberal reformer under the July Monarchy and a leader of the “banquet campaign” that produced the Revolution of 1848, condemned the French judicial system in 1871: “No, the magistrature, such as it is constituted, does not offer sufficient guarantees to the protection of law and rights in our country; the best men within it are lost in the immensities of the judicial ranks and are paralyzed by a vicious organization and by a mode of procedure more vicious still.”¹ The main target of this blunt statement was the method by which magistrates were promoted, a method that effectively immersed the judicial bureaucracy, and occasionally justice itself, in favoritism and politics. It was Barrot’s hope—and that of many others, for the critics of the judicial system were not few—that the new Third Republic might end the abuses practiced by the various royal, imperial, and republican regimes of the first seven decades of the nineteenth century. But the next forty-four years, until the First World War, presented little evidence that this hope would be realized under the Third Republic. Indeed, by attempting to purge the magistrature of judges and prosecutors whose republican loyalty was suspect, the new regime exacerbated the abuses Barrot denounced.

The court structure in France was highly complex and can be confusing to Americans unfamiliar with any judicial system outside the Anglo-Saxon tradition.² Twenty-six *cours d’appel* served the eighty-seven departments of the nation and

The Mellon Faculty Development Fund of West Virginia Wesleyan College provided financial support for the research on this project. Karen M. Offen read the manuscript in draft and contributed valuable critical suggestions; research assistant Carol Louise Bird prepared the charts and helped with the arcana of computerizing relevant data.

¹ Barrot, *De l’organisation judiciaire en France* (Paris, 1871), 12. For other expressions of the reform that a republic might bring, see Lucien-Anatole Prévost-Paradol, *La France nouvelle* (Paris, 1868); Jules Favre, *De la réforme judiciaire* (Paris, 1877); George Picot, *La Réforme judiciaire en France* (Paris, 1881); M. Ricard, *Des devoirs des magistrats* (Rouen, 1881); Paul Philouze, *Études sur l’organisation de nos institutions judiciaires* (Paris, 1882); Eugène Pompei, *Du rôle de la magistrature dans une république* (Montpellier, 1885); and Ernest Rouillet, *Des devoirs des magistrats* (Lyon, 1886).

² For an introduction to the French judicial system as a whole, see, in particular, Raoul de La Grasserie, *De la justice en France et à l’étranger au XX^e siècle*, 3 vols. (Paris, 1914); René David and Henry P. de Vries, *The French Legal System: An Introduction to Civil Law Systems* (New York, 1958); R. C. K. Ensor, *Courts and Judges in France, Germany, and England* (Oxford, 1933); and Thomas John Schick, “The Parisian Court of Appeals and Its Magistrature, 1848–1914” (Ph.D. dissertation, Columbia University, 1977). Also valuable for background are Pierre Bouchardon, *Le Magistrat* (Paris, 1929); André Cambréal, *Le jury criminel: Comment se forme, délibère, et*

TABLE 1
The Courts of Paris in the Third Republic

<i>Court</i>	<i>Bench</i>	<i>Parquet</i>
COUR DE CASSATION	<p><i>Premier Président</i> (Chief Justice)</p> <p>3 <i>Présidents de Chambre</i> (Presiding Justices), 1 per Chamber</p> <p>45 <i>Conseillers</i> (Associate Justices), 15 per Chamber</p>	<p><i>Procureur Général</i> (Attorney General)</p> <p>6 <i>Avocats Généraux</i> (Solicitors General)</p>
COUR D'APPEL DE PARIS	<p><i>Premier Président</i> (Chief Justice)</p> <p>12 <i>Présidents de Chambre</i> (Presiding Justices), 1 per Chamber</p> <p>77 <i>Conseillers</i> (Associate Justices), 6 per Chamber¹</p>	<p><i>Procureur Général</i> (Attorney General)</p> <p>10 <i>Avocats Généraux</i> (Solicitors General)</p> <p>16 <i>Substituts Généraux</i> (Assistant Attorneys General)</p>
TRIBUNAL DE LA SEINE	<p><i>Président</i> (Chief Judge)</p> <p>17 <i>Vice-Présidents</i> (Presiding Judges), 1 per Chamber</p> <p>17 <i>Présidents de la Section</i> (Associate Judges), next in line to preside</p> <p>105 <i>Juges</i> (Associates Judges), 6 per Chamber²</p> <p>40 <i>Juges d'Instruction</i> ("Examining Magistrates")</p>	<p><i>Procureur de la République</i> (District Attorney)</p> <p>47 <i>Substituts</i> (Assistant Prosecutors)</p>

¹ Includes 5 replacements.

² Includes 3 replacements.

were responsible for the most serious aspects of both civil and criminal justice. The most important *cour d'appel* was that of Paris, not only because it sat in the capital but also because seven departments (two more than for any other appellate court) came within its jurisdiction (*ressort*). Each of these courts was divided into permanent standing chambers to impose judgment in various types of cases. One or more *chambres civiles* heard appeals from the lower civil and commercial courts (*tribunaux de première instance* and *tribunaux de commerce*); a *chambre criminelle* heard appeals from the tribunals that tried misdemeanors (*cours correctionnelles*); the *chambre des mise en accusation* acted as a grand jury within its jurisdiction, handing out indictments; and a *chambre des vacances* only sat during the annual court recess from August 15 to October 15. Each chamber had its own panel of justices made up of a presiding justice (*président de chambre*) and a varying number of associate justices (*conseillers*), one of whom was designated vice-president of the panel. The chief justice (*premier président*) of each appellate court could sit with any of the chambers under his jurisdiction and, when present, presided.

For criminal justice, the bench of the appellate court composed the *cours d'assises*, which tried felonies (*crimes*) committed in the town in which the court sat, and provided the presiding justice for a panel of judges from a lower tribunal whenever the assizes met in another town within its jurisdiction. A panel of three judges heard cases at assize; the presiding justice was named by the minister of justice in Paris, and the two associate justices were selected by the chief justice of the appellate court for each jurisdiction. Sitting as an assizes court, the justices themselves did not decide any case before them: a jury rendered the verdict, and the justices only imposed the sentence. But the presiding justice of the assizes court had enormous power over the proceedings before the case went to the jury for deliberation.

The bench (*magistrature assise*) of the appellate court had its counterpart in the parquet (*magistrature debout*) within each jurisdiction. An attorney general (*procureur général*) had the responsibility of overseeing all prosecutions. He was seconded by assistant attorneys general (*substituts généraux*), who attended to the details of individual investigations, and by the solicitors general (*avocats généraux*), who actually presented the cases before the court. The parquet was further augmented by the peculiarly French institution of the *juge d'instruction*, for which the traditional English translation—"examining magistrate"—is inadequate. The *juge d'instruction* occupied a position between the *magistrature assise* and the *magistrature debout*: he sat on a lower court, the *tribunal de première instance*, but with the parquet of the appellate court; he directed the investigation of felonies. (For the appellate and other courts of Paris, see Table 1.)

One of the cherished anecdotes of French baccalaureate examinations is the

statue le jury de cour d'assises (Paris, 1937); Louis Casamayor, *La Justice, l'homme, et la liberté* (Grenoble, 1964), and *Le Bras séculier: Justice et police* (Paris, 1960); Henri Chardon, *L'Administration de la France: Les Fonctionnaires du gouvernement—Le Ministère de la justice* (Paris, 1908); Robert Charvin, *Justice et politique: Évolution de leurs rapports* (Paris, 1968); Jean-Louis Costa, *Liberté, ordre public, et justice en France*, 3 vols. (Paris, 1967); Maurice Garçon, *Histoire de la justice sous la Troisième République*, 3 vols. (Paris, 1957); Georges Guilhermet, *Souvenirs d'un avocat de la Belle Époque* (Paris, 1952); Otto Kirchheimer, *Political Justice: The Use of Legal Procedure for Political Ends* (Princeton, 1961); and Marcel Rousselet, *Histoire de la magistrature française des origines à nos jours*, 2 vols. (Paris, 1957). The *Almanach National*, 1871–1914, is a valuable source for judicial structure and organization.



Figure 1: "View of Faces—The Grand Staircase at the Palace of Justice," by Honoré Daumier (1808–79). Although drawn in the middle of the nineteenth century, this depiction of the "men of justice" also typifies the magistrature of the Third Republic. Lithograph first published in *Le Charivari*, February 8, 1848.

question, "Who is the most powerful man in France?" Candidates invariably reply, "The president of the Republic." The correct answer is the *juge d'instruction*. Before the powers of his office were minimally circumscribed by the law of December 8, 1897, the *juge d'instruction*, even more than the *parquet*, could order searches, seizures, and arrests on his own authority and without fear of being countermanded. He operated as a one-man grand jury, with the power to order a case before the *chambre des mise en accusation*, which produced a *pro forma* indictment, or to dismiss the charges entirely. During the time that he conducted his investigation of a case (the *instruction*), he could keep all of the details secret from the defense and hold the accused in custody, without access to an attorney, for as long as he chose. After 1897, the *juge d'instruction* had to bring charges against those he arrested within twenty-four hours (a French version of *habeas corpus*), had to advise those arrested of their right to legal counsel and to provide summaries of his investigations to the defense, and could hold an accused incommunicado for only ten days (renewable once), except for visits by the accused's attorney. These were extraordinary powers to place in the hands of a magistrate at a relatively early stage in his career and at a relatively low rank on the promotion scale. R. C. K. Ensor has cogently summarized the potential for mischief in this situation: the *juge d'instruction* "represents one of the points at which anyone concerned to exert improper influence over the judicature is most likely to bring it to bear—the more hopefully, because the post is not a very high one, and its occupant is apt to be of an age when promotion means a great deal to him."³

Below the appellate courts were 359 local tribunals (*tribunaux de première instance*). As in the case of the appellate courts, the most important tribunal was that of the Seine. Located in Paris, the Tribunal de la Seine included in its jurisdiction all of the *arrondissements* of the capital and its *banlieue*. Some judges of these tribunals heard civil cases, and the number of judges in the civil chambers varied, depending upon the court and its location; other chambers consisted of three-judge panels for the *cours correctionnelles* to judge cases involving misdemeanors (*délits*). (The associate justices for the *cours d'assises* that did not meet in the home cities of the appellate courts were also drawn from the chambers of the tribunals.) The presiding judge of each chamber held the title vice-president of the tribunal, and, again like the appellate courts, the chief judge of the tribunal (*président*) could sit in, and preside over, any chamber he wished. The other members of the chambers were simply called *juges*. The *parquet* of the tribunal consisted of a district attorney (*procureur de la République*) and his assistants (*substituts*). There was no jury for the tribunals, and an appeal of a decision of the chambers could be taken only to an appellate court.

Above the appellate courts stood the *cour de cassation*, the court of final appeal. Although it heard cases on points of law, the *cour de cassation* could not be considered a French "supreme court," because it did not have the power to reverse lower-court verdicts. Instead, it was limited to "breaking" (*casser*; thus the name) a previous decision and requiring a retrial at the original jurisdiction. The *cour de cassation* was organized in three chambers, one each for civil cases, for criminal

³ Ensor, *Courts and Judges in France, Germany, and England*, 35.

cases, and for the decision of which cases to accept for review. A chamber consisted of a presiding justice (*président de chambre*) and fifteen associate justices (*conseillers*); the chief justice (*premier président*) could sit with any chamber and, when present, presided. The parquet of the court was composed of the attorney general (*procureur général*) and six solicitors general (*avocats généraux*) to make presentations before the bench. Cases could be referred to the court by the defense, by the prosecution, or by the minister of justice, who could act “in the interest of law” to request a clarification or interpretation even if the case at hand was moot. Finally, the *cour de cassation* acted as the disciplinary board for any magistrate accused of malfeasance or dereliction of duty.

DURING THE THIRD REPUBLIC, entrance into the ranks of the magistrature was relatively easy. Until 1908, the requirements remained identical to those of previous regimes in the nineteenth century. The candidate had to complete his *licence en droit*, serve a twelve-month clerkship (*stage d'avocat*), pass an unchallenging *examen d'aptitude*, reach the age of twenty-five, and win recommendation to the Ministry of Justice from a chief judge or justice. Such a recommendation usually required at least limited political influence. After 1908, the candidate faced a second examination, but this test was in no sense a *concours*: almost everyone passed it. A few scholarship students at the law schools pursued careers in the magistrature, but most eager young judges and prosecutors came from bourgeois families that could afford to provide them with a professional education and discouraged deviation from *idées reçues*, *bien pensant*, and *fil à papa*. An outside income was almost imperative. The starting salary for the lowest ranks was only 2,400 FF per year, and magistrates were expected to maintain a “proper” image. When Jules Favre wrote in 1877 that only the rich could afford to be magistrates, he did not greatly exaggerate.⁴

The framework of courts and the ranks of the magistrature were overseen by the minister of justice at the Place Vendôme.⁵ The ministry traced its lineage to the office of chancellor under the *ancien régime*, but it more properly dated from a series of decrees in 1791 that established its modern functions: to register the laws (“keep the seals”), to publish and send out these laws to the jurisdictions, and to maintain correspondence with, and direction of, the system of courts. The Revolutionary and Napoleonic periods brought numerous reorganizations, as both Jacobins and Bonapartists realized the importance of controlling the administration of justice. Napoleon even made the minister of justice *grand juge*, enabling him to sit as chief justice of the Cour de Cassation or of any appellate court as the needs of the state required. Nevertheless, the changes did not fundamentally alter the structure

⁴ Favre, *De la réforme judiciaire*, 48. On the subject of magistrates, their recruitment, and their services, see Martial Bergeron, *La Réforme de la magistrature* (Paris, 1908); Jacques du Boishamon, *Du recrutement de la magistrature* (Paris, 1902); Maurice Dehesdin, *Étude sur le recrutement et l'avancement des magistrats* (Paris, 1908); G. Demartial, *La Nomination des magistrats* (Paris, 1907); Paul Lallemand, *Le Recrutement des juges* (Paris, 1936); F. L. Malepeyre, *La Magistrature en France et projets de réforme* (Paris, 1900); Georges Marchand, *Le Recrutement de la magistrature en France* (Paris, 1910); and Jean Mendioudou, *Étude des projets de réforme de la magistrature sous la Troisième République* (Paris, 1912).

⁵ For the Ministry of Justice, see Pascal Durand-Barthez, *Histoire des structures du ministère de la justice, 1789–1945* (Paris, 1973); and Louis Rouvier, *La Chancellerie et les sceaux de France* (2d edn., Marseille, 1950).

set down in 1791. Instead, the ministry was confirmed in its functions and began to develop a bureaucracy to meet them. The most important aspect of this evolution was the creation of two great divisions within the ministry, the *direction des affaires civiles* and the *direction des affaires criminelles*, each under the control of a *chef du bureau* hand-picked by the minister and ranking immediately beneath him in the hierarchy. The decision to divide the administration of justice so high in the ministry's bureaucratic structure had significant ramifications. It meant that the judges and justices, who heard both civil and criminal cases, were subject to quite different pressures from the ministry, depending upon the jurisdiction of a given case. In addition, the directors of the ministry's civil and criminal divisions came to have enormous power concentrated in their hands.

The remaining structure of the ministry had also clearly emerged by the beginning of the nineteenth century, although it underwent various modifications before 1914. Initially, the *chef du cabinet* of the minister acted as an intermediary between the minister and the divisions of civil and criminal affairs and the departments of accounting and personnel. Between 1816 and 1884, this intermediary position allowed the *chef du cabinet* to build a base of power in what was termed the *secrétariat général*, a development that served to undermine the position of the directors for civil and criminal affairs. Ministerial decrees of December 30, 1884, and February 1, 1885, first curbed the growth of the *secrétariat général* and then replaced it with the *cabinet du ministre*, in which the division and department chiefs sat with the *chef du cabinet*. The last major change before 1914 came on June 9, 1909, when the departments of personnel and accounting were merged in the *direction du personnel*, and the *cabinet du ministre* was changed to the *direction du ministre*, an alteration in name only. Despite the increased responsibility of the accounts, the chief of personnel remained a lesser figure than his colleagues in civil and criminal affairs. Indeed, the department of personnel was viewed as a training ground for successors to those more demanding, independent, and politically delicate *directions*.

Officials in the bureaucracy at the Place Vendôme never numbered many more than one hundred before 1914 and worked only from 11 A.M. to 5 P.M. In ascending order, the positions were those of copying clerk (*expéditionnaire*), clerk (*commis*), head clerk (*rédacteur*), deputy bureau chief (*sous-chef*), and bureau chief (*chef du bureau*)—that is, head of a department or division. Magistrates were usually chosen to fill the principal positions, those of bureau and deputy bureau chiefs, with political expediency the most important qualification—although every magistrate had a *licence en droit*. Prior to June 5, 1909, the other positions were not filled through a competitive examination but did have educational prerequisites: the copying clerks and clerks were required to complete the baccalaureate and serve a twelve-month probationary period in a legal office; head clerks were required to have the *licence en droit* and to complete a formal clerkship, as was mandated for magistrates. After 1909, a competitive examination was established for all ranks through head clerk, but, to compete for head clerk, a candidate had to serve two years with the ministry in a lower position or to be a magistrate. Thus, the Ministry of Justice defended its median and higher ranks from outside infiltration, while bowing to demands for competitive placement.

The French system of justice created a great distinction between the bar and the magistrature. Law students usually decided by the age of twenty-five which direction their careers were to take; cross-overs were infrequent. An esprit de corps as well as bureaucratic unity thus remained strong among the approximately forty-five hundred magistrates, a figure that did not vary greatly between 1871 and 1914. The positions on the bench and the parquet were all but guaranteed. At the same time, since the magistrature was clearly part of the state bureaucracy, the magistrates were not commonly regarded, among themselves or by outsiders, as independent. Rather than staking out precedents, judges relied on the text of the law codes. A panel of at least three judges heard a case and rendered its verdict without public dissenting opinions. Associate justices frequently felt compelled to defer to the wishes of the chief justice because he alone recommended promotions for the bench. Prosecutors had even less space for independence. The twenty-six attorneys general of the appellate courts were accountable directly to the minister of justice. Within each jurisdiction, parquet officers reported to the attorney general and were dependent on his recommendation for promotion. None of the members of the parquet had the benefit of irremovability, as did the judges and justices. A prosecutor who incurred the displeasure of his superiors could be ejected from the magistrature, but, more often, he was assigned to the bench at the lowest rank and could consider his career broken.

The desire for promotion and fear of dismissal dominated the mind of many a magistrate. Most often, his first positions were on the parquet or bench of a provincial tribunal. The inadequate salary and backwater society contrasted acutely with the comfortable world in which he had been reared and educated. He could dream of a brilliant career, culminating in appointment to the Cour d'Appel de Paris or even the Cour de Cassation, but that required rapid promotions. He learned that advancement came fastest to magistrates who were part of the parquet and that it was not difficult to shift from one side of the bench to the other and back again. The caveat was that service on the parquet carried with it the risk of dismissal from which the bench was largely exempt. To facilitate promotion, a young magistrate had to maintain at least "correct" relations with the chief justice and the attorney general of his jurisdiction, because mediocre or indifferent recommendations from them were difficult to overcome. Naturally, he began to develop that exaggerated "respect" for superiors and aversion to "wrong" ideas so characteristic of the ambitious in the French magistrature. He had only to remember the law of August 30, 1883, that suspended for three months the irremovability of judges. This experience warned that the principle of permanence in office for seated magistrates, established by the laws of 1810 and 1852, could be abolished at any moment. Those laws had created tenure for justices and judges, except in instances of insanity or incapacitating illness, while also setting the mandatory retirement age at seventy (seventy-five for the Cour de Cassation). The statute of 1883 empowered the Cour de Cassation to expel from the ranks any magistrate who manifested hostility to the principles or form of republican government: 614 judges and justices were summarily so expelled.⁶

⁶ On these issues, see Albert Desjardins, *L'Inamovibilité de la magistrature dans l'ancienne France* (Paris, 1890), and *La Magistrature élue* (Beauvais, 1882); Henri Fourchy, *Observations sur la suspension de l'inamovibilité de la*

The law of 1883 provides the most dramatic evidence that political allegiance could make or unmake careers in the magistrature. The Republicans, both Opportunist and Radical, who since 1879 had controlled the Chamber of Deputies, the Senate, and the presidency, wanted to move against judges who refused to enforce the new anticlerical laws, especially those of March 29, 1880, aimed at the religious congregations. Between April 1880 and August 1883, 226 magistrates on the parquets resigned rather than carry out prosecutions against the Jesuits and other male religious orders. About an equal number created delays or obstructions to the prosecutions. Similarly, approximately four hundred judges and justices refused to order enforcement of the laws. Some of this recalcitrance arose from sincerely held religious conviction; some of it derived from antirepublican sentiment among magistrates who had been appointed during the imperial regime of Louis Napoleon and who hoped for a return of the Bonapartes or for a royalist restoration. Understandably, the Republicans determined to remove their enemies from the magistrature. Beginning in February 1879 with Philippe Le Royer as minister of justice and continuing to December 1882 through the ministries of Théodore Cazot and Gustave Humbert, the parquets were thoroughly purged: 82 percent of the prosecutors (1,763 out of 2,149) were dismissed or replaced.⁷

There remained the magistrates of the bench. Like the clergy, the magistrature was an easy target for Republican legislators, who associated it with the repression of the Second Empire. Jules Simon, a moderate Republican senator, did note one difference: hatred of the clergy was collective, that of the magistrature individual.⁸ Each deputy and each senator had his chosen victim. When the law of 1883 was passed with the provision that as many as 614 “irremovable” magistrates might have their appointments revoked, the minister of justice, Félix Martin-Feuillé, had a difficult time choosing. Denunciations from legislators, from prefects, even from Radical newspapers, filled the ministry. One expedient was to dismiss the most visible: 10 of 26 chief justices of the appellate courts and 117 of 359 chief judges of the tribunals were removed. At Angers, the associate justices rallied behind the chief justice, with the result that twenty-one of the twenty-three members of the court were replaced. Monarchists in the legislature alleged that many magistrates were dismissed merely for being practicing Catholics and therefore suspect as “clericals.” When questioned before the Senate in December 1883, Martin-Feuillé did not deny the charge.⁹

For magistrates who put promotion above principle, the ideological battles of the Third Republic made it difficult to know where to place their allegiance. Opportunist Republican ministers were likely to reward Opportunist Republican magistrates, less likely to reward Radicals, and very unlikely to reward monarchists. Because the

magistrature (Paris, 1882); Victor Jeanvrot, *La Magistrature, l'immovibilité* (Paris, 1882); Eugène Ninard, *De l'immovibilité de la magistrature et de l'indépendance du magistrat* (Bordeaux, 1884); B. Schader, *Nos magistrats, ce qu'ils seront par le suffrage universel* (Paris, 1882), and *Encore nos magistrats, ce qu'ils seront par le suffrage universel* (Paris, 1882); G. Vidal, *De l'immovibilité de la magistrature française et de son institution en 1849* (Paris, 1850); and Auguste Vidalin, *Des inconvénients et des avantages de l'immovibilité dans la magistrature* (Colmar, 1848).

⁷ Georges Picot, *La Magistrature et la démocratie, une épuration radicale* (Paris, 1884), esp. 6–52; and Georges Legée, *La Loi du 30 août 1883 sur la magistrature* (Paris, 1904), esp. 83–102.

⁸ Simon, as quoted by Fourchy, *Observations sur la suspension*, 7.

⁹ Sénat, *Débats Parlementaires, Journal officiel*, December 26, 1883, question from Ernest Denormandie.

Third Republic saw numerous shifts of power among these contending political groups, the stakes of loyalty were high. They weighed most heavily on the parquet, which had to conduct the prosecution in sensitive cases. The bench also faced crises of conscience. Most of the truly important politicians were members of the bar and practiced before the bench. Judges depended ultimately upon the minister of justice for promotion; the minister of justice depended upon these powerful politicians for legislative support. There was always the implied threat that a decision rendered against the clients of an important politician-attorney could destroy forever the promotion chances of a magistrate.

Under the best of circumstances, the promotion of magistrates during the nineteenth century was a capricious exercise dominated by patronage and favoritism. This in itself was an advance over the venality of the *ancien régime*, when magisterial positions were considered proprietary and hereditary. But the step was hardly great, because before 1908 almost no rules governed the promotion process. Odilon Barrot declared bluntly in 1871, "I have known magistrates whose appointment to the Cour de Cassation can only be attributed to political influence."¹⁰ Thirty-one years later, in 1902, a young jurist, Jacques du Boishamon, expressed a broadly held opinion within the judicial world when he wrote in his doctoral thesis, "We have seen numerous abuses committed by unscrupulous ministers of justice who sacrificed recognized merit to incapacity recommended by the influential or to fawning subservience to the current government."¹¹ Foremost among the complaints was the charge that the minister of justice, with the advice of his chief assistants and pressured by the importuning of deputies and senators for their favorites, advanced magistrates as he chose, without oversight and without accountability.

Of the forty men who filled the office of minister of justice from 1871 to 1914, 68 percent were politician-attorneys and only 23 percent were from the magistrature. It is frequently supposed that stability and continuity were greater among top ministerial aides, but this is not entirely true. During the same period, there were thirteen different directors of civil affairs, twenty-six directors of criminal affairs, and thirty-nine directors of personnel. Almost invariably, these assistants were far more conversant with the personnel files than the minister, but the dossiers reveal that the minister himself made the final decisions on promotions to all important positions. Memoranda in the files also indicate that assistants were as willing as ministers of justice to set aside merit in favor of patronage with regard to the promotion of friends or political allies—although the ministers and assistants did not always agree on specific individuals.

The reforms initiated by the ministerial decree of February 13, 1908 (prepared by that of August 18, 1906), were largely cosmetic, designed to divert the continuing, ever-louder criticism of the promotion process. Careers were made less rapidly after 1908, but room for discretionary advancement was hardly reduced. Mere inscription upon the newly devised table of advancement did not mean

¹⁰ Barrot, *De l'organisation judiciaire en France*, 105.

¹¹ Boishamon, *Du recrutement de la magistrature*, 146.

promotion. The political games played out in secret at the ministry on the Place Vendôme remained the same. Proper patronage could ensure a brilliant career in which each promotion leap-frogged the fortunate magistrate several ranks and brought him to the politically central courts of Paris. Without it, even the most talented judge or prosecutor was doomed to a slow climb within the ranks, at best, and a long residence in the provinces.

THE WAY IN WHICH THE GAME OF PROMOTION WAS PLAYED can be judged in detail from the personnel dossiers of the magistrates, which are now held in the Archives Nationales, series BB 6 II. The dossiers were created in 1848 to record the progress of individual magistrates and to hold evaluations, letters of recommendation, and memoranda about their suitability for advancement. The series is divided into three parts: cartons 1 to 434 contain the dossiers of magistrates who retired between 1842 and 1883; cartons 435 to 611, the dossiers of magistrates who retired between 1883 and 1900; and cartons 612 to 1294, the dossiers of magistrates who retired between 1900 and 1942. Under a law of 1979 that revised the rules for the use of archival material, only the first two parts and the dossier of any magistrate in the third part born at least 125 years before the request are freely accessible. Access to the remaining dossiers of the third part is subject to special application.¹²

Although many Republicans may have believed that they could, as Odilon Barrot had urged in 1871, create a magistrature in which merit and not patronage or favoritism would be the basis of promotion, the personnel dossiers present ample evidence that they did not carry out this goal. From the birth of the Third Republic, and particularly after their capture of the vital organs of the state in 1879, the Republicans had to defend the regime from its monarchist and clerical enemies. The Seize Mai ministry of Orleanist Albert de Broglie, the plots of royalist and clerical extremists such as Baron Athanase de Charette, and Boulangism did not appear in the 1870s and 1880s to be so utterly lacking in political reality as they do in retrospect. Ideals had to give way to stern practicality. As Henri Albert Dauphin, who was promoted to attorney general of the Cour d'Appel de Paris in February 1879 and who directed from there the first purge of antirepublicans, recalled, "I entered the magistrature because I was a politician [*homme politique*], and, at that time, the government needed politicians at the head of the great parquets."¹³ During the next ten years, the Republic was defended and a "republican system of justice" created, as clericals, monarchists, and even those whose political opinions were too vague to be calculated exactly were systematically replaced.¹⁴ Republicans

¹² Mme. Ségolène de Dainville-Barbiche is the archivist for the BB 6 II series.

¹³ Dauphin to Eugène Guérin, minister of justice, April 22, 1893, in Archives Nationales [hereafter, AN], BB 6 II, 480, dossier of [hereafter, dos.] Henri Albert Dauphin.

¹⁴ For examples of the treatment of "clericals," see AN, BB 6 II, 447, dos. Xavier Marie Thomas de Cantorbury Béquet; *ibid.*, 452, dos. Marcel Victor Marie Bilard; and *ibid.*, 531, dos. Pierre François Gustave de La Gorce. For the treatment of monarchists, see *ibid.*, 550, dos. Jean Léopold de Masfrand; *ibid.*, 600, dos. Placide Léopold Thoreau La Salle; and *ibid.*, 447, dos. Jean Marie Louis Vincent Hubert Bécane. For the treatment of magistrates with ambiguous political views, see *ibid.*, 467, dos. Pierre Chaize; *ibid.*, 559, dos. François Émile Moreau; and *ibid.*, 577, dos. Henri Auguste Xavier Prinnet.

who had themselves been purged from the magistrature during the Seize Mai crisis were rehabilitated.¹⁵ Questions of merit were made secondary to political reliability. The litmus test was the phrase “dévoué aux institutions républicaines,” and it came to cover even the sins of drunkenness, sexual misconduct, and bankruptcy—all of which would normally have occasioned harsh disciplinary action.¹⁶ However necessary to the firm establishment of republican government these methods might have been before 1890, they were an infection in the magistrature that became deep-seated. One measure of the debilitation that was the result of continued favoritism, political patronage, nepotism, and overlooked questionable morality can be taken from an examination of the careers of eleven magistrates, born between 1846 and 1864, men who came of age with the solidification of the Third Republic.

Félix Pierre Joseph Leydet (1855–1919) began his career as a head clerk in the Ministry of Justice in 1877. Eight years later, in 1885, he requested assignment to the court system and was made *juge d'instruction* on the tribunal at Nantes.¹⁷ This move began a series of advances that thirteen years later brought him back to Paris: district attorney at Dreux in 1887, district attorney at Pontoise in 1893, and assistant prosecutor on the parquet of the Tribunal de la Seine in 1898. Two years later, he was made *juge d'instruction* in Paris, a position preferable to technically superior ones on appellate courts in the provinces. His rise had not been spectacular—he was forty-five at this point—but he had made a successful career. The evaluations of his superiors spoke not to any outstanding ability, which was never cited, but rather to his politics (“good Republican”) and his connections (his wife’s father was a deputy from the Eure-et-Loir).¹⁸

As a *juge d'instruction* in the capital, Leydet investigated felonies brought before the Cour d'Assises de la Seine. During the next seven years, he demonstrated considerable dexterity in handling cases with political overtones. Important Radical Republican politicians, Gaston Thomson at the naval ministry and Jean Guyot-Dessaigne, minister of justice from 1906 to 1908, praised his ability to prepare prosecutions while simultaneously burying scandals: “en évitant les indiscretions pour les faits essentiels.” In 1907, Victor Fabre, attorney general of the Cour d'Appel de Paris, proposed Leydet for promotion to one of the seventeen vice-presidencies of the Tribunal de la Seine, a post usually considered a training ground for elevation to the bench Fabre faced.¹⁹ Leydet was passed over in 1907, and in 1908 he ruined his career in trying to protect his mistress, the notorious Meg Steinheil, who was accused of the double murder of her husband and her mother. Leydet arranged to be assigned to the case and contrived to overlook evidence against Mme. Steinheil in what was admittedly a baffling mystery. Unluckily for

¹⁵ For examples of this rehabilitation, see AN, BB 6 II, 500, dos. Jean Marie Léon Faynot; *ibid.*, dos. Joseph Paulin Guisol; and *ibid.*, 549, dos. Georges Martin-Sarzeaud.

¹⁶ For examples of the treatment accorded those accused of drunkenness, sexual misconduct, and bankruptcy, see AN, BB 6 II, 484, dos. Charles Alfred Delacour; *ibid.*, 449, dos. Jean Charles Paul Béra; and *ibid.*, 549, dos. Jean Antoine Gustave Martin.

¹⁷ AN, BB 6 II, 1025, dos. Félix Pierre Joseph Leydet.

¹⁸ *Ibid.*: annual evaluations of 1887, 1892, 1896.

¹⁹ *Ibid.*: Thomson notation, December 18, 1907; Guyot-Dessaigne notation, February 10, 1907; and Fabre recommendation, August 12, 1907.

Leydet, his favoritism came to the attention of the newspapers, and he was removed from the investigation. The press called for his dismissal, but Leydet's friends within the magistrature, Fabre foremost, convinced the then minister of justice, Aristide Briand, to allow Leydet to accept a reduction in rank to simple judge on the Tribunal de la Seine.²⁰

Although Mme. Steinheil was acquitted of murder, the scandal of "le Leydet de Meg" would not die. Leydet, who seemed so close to great stature in 1907, was a pariah by 1909. His health deteriorated, and two years later he was reduced to writing a pathetic letter to Alfred Giraud, the new minister of justice, begging for any promotion. Henri Ditte, chief judge of the Tribunal de la Seine, seconded this appeal with a letter noting that Leydet had "cruelly paid for his mistake." In the ministry's opinion, he was "a magistrate of mediocre value, of feeble authority."²¹ A year later, the painful request was renewed, with Leydet writing to Briand, once again the minister of justice, "I want to tell you what hopes I place in your good will and also how much happiness I will owe to an elevation." Briand, who had permitted Leydet his face-saving reduction in 1909, again took pity and raised him to section president on the Tribunal de la Seine, the smallest advance possible and not even an official promotion. The First World War opened many places in the ranks of the magistrates. Elevations came to many, and often quickly, but not to Leydet.²² He died in 1919, a testimony to how the system could reward those with patronage but could just as rapidly abandon those who had been irremediably indiscreet.

The career of Leydet's champion, Victor Albin Fabre (1852–1916), is also illustrative of this process.²³ Despite serious defects in character and judgment that became evident later, Fabre was appointed assistant prosecutor on the tribunal at Digne in 1879 after Charles Bessat, the attorney general at Aix, pronounced him "one of the excellent examples of our Republican youth." His promotion thereafter was extremely rapid: assistant prosecutor of the Cour d'Appel d'Aix in 1880, district attorney at Draguignan later that year, and solicitor general at Aix in 1882. The Ministry of Justice, firmly in the hands of the Republicans, took particular note that Fabre "belonged to a family absolutely devoted to the government." This pedigree outweighed reports that he was "violent, passionate, without education, devoured by ambition, imprudent in his remarks, incapable of discretion, excessive in his acts."²⁴

In 1892, these criticisms seemed justified. Fabre's wife sued him for divorce, charging that he had seduced her twenty-two-year-old sister, Jeanne. The former Mme. Fabre, née Marie Callamand, publicized her allegations broadly, and, by

²⁰ *Ibid.*: notations, October 15 and December 17, 1908; and Fabre's comment on the issue, March 24, 1909. For additional information, see AN, BB 18, 2369, Affaire Steinheil. The roles played by Leydet and other magistrates in the cases of Marguerite Steinheil, Thérèse Humbert, and Henriette Caillaux are the subject of my book-length manuscript, "Politics and the Courts in Belle Époque France," which is in preparation.

²¹ AN, BB 6 II, 1025, dos. Leydet: clipping from *L'Action française*, September 29, 1909; Leydet to Giraud, January 31, 1911; Ditte to Giraud, February 15, 1911; and annual evaluation of 1912.

²² *Ibid.*: Leydet to Briand, October 7, 1912; and notation of 1918.

²³ AN, BB 6 II, 850, dos. Victor Albin Fabre.

²⁴ *Ibid.*: Bessat notation, March 17, 1879; annual evaluation of 1892; and notation, August 17, 1886.

early 1893, Fabre's authority in Aix had been seriously eroded. The situation was immensely complicated by a bitter custody fight between the two over their daughter. When it appeared that Fabre's fellow magistrates might ensure his victory in the dispute, Marie Callamand threatened to shoot her former husband. In addition, Jeanne was planning to marry a certain de Belval, whose monarchist connections were certain to exploit her tales of Republican moral turpitude. It was imperative to move Fabre to another post, but, protesting his complete innocence of any of the "infamies against him," he refused to accept a change of position unless it had the semblance of a promotion. Finally, in June 1893, he was appointed a judge on the Tribunal de la Seine.²⁵

Fabre's political allies made certain that his advancement was not long delayed by the minor inconvenience of scandal. He was elevated to vice-president of the tribunal in 1898 and then to solicitor general of the Cour d'Appel de Paris in 1901. At that time, Fabre wrote to the minister of justice, Ernest Monis, "Thanks to you, I am finally going to participate wholeheartedly in the defense of Republican institutions. I have always thought that a magistrate could do that, without forgetting any of his duties to justice."²⁶ Such sentiments won him a place in the highest ranks of the ministry, as director of civil affairs in 1904. The same year, he was given the powerful position of district attorney for the Tribunal de la Seine. Two years later, he reached the zenith of his prestige by attaining the post of attorney general of the Cour d'Appel de Paris.

Fabre's brilliant career was shipwrecked on the Rochette affair. In 1911, for complicated reasons that smacked of corruption, the premier, Ernest Monis, to whom Fabre owed his promotion, and the finance minister, Joseph Caillaux, requested that Fabre arrange for an extraordinary postponement in the appeal hearing of financier Henri Rochette. Fabre bent to their pressure but was dismayed at this particular defense of "Republican institutions." He prepared a memorandum giving his perception of the incident, which he placed in the hands of Briand, the minister of justice and an enemy of Caillaux. Briand did not file the memorandum but turned it over to his successor, Louis Barthou, who also had little love for the finance minister. During a vicious press campaign against Caillaux by *Le Figaro* in 1914, the existence of this memorandum was alluded to several times. After Caillaux's wife, Henriette, shot and killed *Le Figaro's* editor, ostensibly to prevent the revelation, Barthou read the document aloud in the Chamber of Deputies. In the scandal that followed, Fabre was forced to resign and to accept demotion to chief justice of the Cour d'Appel d'Aix. The current minister of justice, Jean Baptiste Bienvenu-Martin, angrily denounced Fabre for having disgraced the magistrature, but whether his offense was bowing to political pressure or revealing that he had done so was unclear.²⁷ A letter to Bienvenu-Martin from Mme. Marie Cjérardin, widow of a distinguished jurist, did not suffer from equivocations or ambiguities:

²⁵ *Ibid.*: notations, letters, and reports on Fabre's marital problems, August 1892 to April 1893. Fabre lost his custody fight in the lower courts but won on appeal.

²⁶ *Ibid.*: Fabre to Monis, March 16, 1901.

²⁷ *Ibid.*: notations of March and April 1914.

[T]he greatest number of evils from which France suffers derives from the failures of justice. Fabre did not carry to his functions the necessary firmness that must belong to any magistrate who exercises his high office. He did not understand his role and task, submitting himself to governmental influences to which he should have had no relation. Thus, it cannot be doubted that he has seriously failed in his duties and that his condemnation to resign his functions is almost certain.²⁸

Fabre himself did not see his own faults so clearly and went as far as retaining an attorney to plead his case with the Ministry of Justice for future promotion to the Cour de Cassation. He appears to have been bitterly disappointed to learn that Bienvenu-Martin would not consider his request.²⁹ He had no opportunity to renew the appeal because his health failed suddenly in 1915, and he died the following year.

The Caillaux affair of 1914 also contributed to the destruction of another magistrate's career, that of Jean Marie Louis Albanel (1854–1927).³⁰ Albanel began as an assistant prosecutor on the tribunal at Sens in 1880 and continued to serve on the parquet at Coulommieu (1882), Paris (1886), and Pau (1888). He moved to Paris permanently in 1896 as a judge on the Tribunal de la Seine. The same year, he was elevated to *juge d'instruction*, a position he held until 1910. He was frequently assigned to investigations that were not of the most sensitive variety but that had within them minor scandals that the Ministry of Justice preferred to have buried. He was, in effect, a political hack. His superiors were not unaware of his value but warned against confiding more difficult cases to him. His work was described as "insufficient" and "superficial."³¹ In addition, he fell into legal difficulties of his own in 1907, in connection with a bitter suit over the dowry of his son's wife. Documents in Albanel's file make it clear that he acted with few scruples in the matter, and he did everything possible to delay a hearing of the suit.³²

Despite this record, Albanel was promoted to associate justice on the Cour d'Appel de Paris in 1910, largely through the influence of two powerful political friends, Jean Cruppi and Joseph Caillaux. Immediately, there was a suspicious change of tone in the evaluations of his work. For the first time in his career, he was fulsomely praised, although his experience on the *magistrature assise* was almost nil. In 1914, he was chosen by the ministry to preside over the trial of Henriette Caillaux for the murder of *Le Figaro*'s editor, Gaston Calmette. The choice was obviously partisan: Albanel owed his promotion to Caillaux, and Caillaux had served in the same cabinet with Bienvenu-Martin, who made the selection. During the trial, Albanel called an unscheduled recess at a moment when it appeared that Mme. Caillaux's defense might collapse. One of the associate justices, Louis Dagoury, angrily turned to Albanel to say, "You dishonor us; you are a wretch!" When *Le Figaro* published the remark, Albanel demanded a formal retraction from

²⁸ *Ibid.*: Cjérardin to Bienvenu-Martin, March 27, 1914. Mme. Cjérardin's letter is a bizarre document to have included in Fabre's file.

²⁹ *Ibid.*: notation of December 27, 1914. Fabre retained Paulin Silvau in his unsuccessful attempt to reach the Cour de Cassation.

³⁰ AN, BB 6 II, 616, dos. Jean Marie Louis Albanel.

³¹ *Ibid.*: notation of 1903.

³² *Ibid.*: documents dated 1899 to 1909.

Dagoury, who refused. A duel was barely avoided. Criticism of the conduct of justice, already widespread, became strident.³³

Albanel received no more promotions. On his retirement in 1921, he asked for the dignity of appointment as an honorary justice (*conseiller honoraire*). The moment was not propitious. Caillaux had been convicted of improper conversations with German agents during the war, and the conservative Bloc National was in power. The Ministry of Justice thus reacted negatively:

The name of M. Albanel is linked in memory to the trial of Mme. Caillaux, over which he presided in the Cour d'Assises de la Seine in July 1914. One recalls the incidents to which it gave place. The best that one can say of M. Albanel is that on this occasion he was unequal to the duties of his office. He called attention to himself through a lamentable lack of authority. He was, in addition, a mediocre magistrate to whom the judicial world has accorded only feeble consideration.³⁴

This opinion notwithstanding, Albanel was made an honorary justice in 1925, when Caillaux's friends returned to power in the Cartel des Gauches.

Mme. Cjérardin's letter to Minister of Justice Bienvenu-Martin in March 1914 condemned the passage of magistrates from the parquet to the bench: the habits learned and the pressures encountered in the *magistrature debout* did not encourage the calm reflection and objectivity necessary for the *magistrature assise*. She could easily have been referring to Albanel, but the specific instance she cited was that of Ferdinand Monier (1859–1919). After fifteen years on the parquets of provincial tribunals and appellate courts, Monier became *chef du cabinet* at the Ministry of Justice in 1898.³⁵ He had in his favor a wealthy wife and the recommendation that “the political devotion of M. Monier is certain; he is sincerely attached to republican institutions.” The notation that he required “more maturity of spirit” was overlooked. From the ministry, Monier moved to a vice-presidency of the Tribunal de la Seine (1898) and to associate justice of the Cour d'Appel de Paris (1903). In 1906, Jean Sarrien asked him to head the department of civil affairs at the ministry, and in the following year, as a reward for work well done, Monier was made district attorney for the Tribunal de la Seine.³⁶ It was his promotion to chief judge of the tribunal in 1911 that occasioned Mme. Cjérardin's criticism.

There were few positions beneath the Cour de Cassation that would have represented a promotion for Monier after 1911, but he secured one in 1916: chief justice of the Cour d'Appel de Paris. This post actually carried more prestige than many of the ranks of the Cour de Cassation, and Monier's career had reached a level rarely attained. He had been made a knight of the Legion of Honor in 1901 and was promoted to officer in 1907 and to commander in 1913. It was, therefore, a shocking assault on the reputation of the magistracy when in November 1917 the Cour de Cassation, acting as the *Conseil supérieure à la magistrature*, ordered his dismissal: Monier had in January 1916 illegally amended a contract between

³³ *Ibid.*: annual evaluation of 1912; and documents dated July 25–31, 1914.

³⁴ *Ibid.*: memorandum of April 12, 1921.

³⁵ AN, BB 6 II, 1083, dos. Ferdinand Monier.

³⁶ *Ibid.*: notations of 1884, 1887, 1896.

Charles Humbert and Paul Bolo-Pasha, both of whom were exposed as German sympathizers; Bolo-Pasha was shot as a traitor.³⁷

Concerning Leydet, Fabre, Albanel, and Monier, the Ministry of Justice and the magistrature as a whole acted with self-righteous hypocrisy. As long as the flaws and indiscretions of a magistrate remained concealed from the great majority of the public, he might advance and prosper, particularly if he had the appropriate ideas and friends. In fact, a healthy propensity toward amorality, not to say immorality, could be useful to the government in sensitive matters. But if a magistrate disgraced “justice,” he could expect little consideration from the ministry or his colleagues. The line between embarrassment and disgrace was not carefully drawn; nor was the identical behavior of different magistrates judged the same way every time. The careers of François Gaston Bonnet (1849–1914) and Paul Adolphe Trouard-Riolle (1857–1922) offer some illumination.

Bonnet, who clearly came from a wealthy and Republican commercial family, held an extremely attractive first appointment as assistant attorney general on the Cour d’Appel de Nancy in 1880.³⁸ Less than six months later, he was elevated to solicitor general at Rennes. Such rapid advancement was rare, but Bonnet was a close friend of René Waldeck-Rousseau, the protégé of Léon Gambetta. Even so, Bonnet chafed at not reaching the heights with even greater dispatch. After four years at Rennes, he shocked the ministry by denouncing his superiors on the appellate court there for “blocking” his career. The attorney general at Rennes could reply only that he had consistently lobbied for Bonnet and had thought him a friend. The pressure was effective because in 1885 Bonnet was made presiding justice of a chamber at the Cour d’Appel de Riom. But Riom was not Paris, and again Bonnet openly protested his treatment two years later. His colleagues complained of his plots and mischief-making (*brouillon*) and of his lack of “the reserve essential in a small town.”³⁹ Bonnet’s high-handedness provoked a serious incident in April 1892, when he pronounced judgment in a case before the court without having heard all of the arguments. The bar of Riom called for his censure, and there was an unpleasant amount of publicity in the local press.⁴⁰ In spite of these blemishes, Bonnet survived, largely because of his political contacts, and finally reached Paris in 1894, when he was appointed an associate justice on the Cour d’Appel de Paris. In 1903, he was advanced to presiding justice of a chamber, and, in 1913, he was named to the Cour de Cassation as an associate justice.

Trouard-Riolle’s career was also marked by questionable incidents and by patronage from the heights of power. It began modestly enough with his appointment as an assistant prosecutor at Rambouillet in 1882, followed by similar posts at Évreux and Le Havre.⁴¹ In 1886, he became an assistant prosecutor on the Tribunal de la Seine. By this time, he had married Marie Lebey, whose father was

³⁷ *Ibid.*: documents from the *Conseil supérieure à la magistrature*, November 6–9, 1917.

³⁸ AN, BB 6 II, 690, dos. François Gaston Bonnet.

³⁹ *Ibid.*: notation of April 15, 1892; and reports of June 1884 and 1887.

⁴⁰ *Ibid.*: reports of April–December 1892.

⁴¹ AN, BB 6 II, 1256, dos. Paul Adolphe Trouard-Riolle.

director of the Agence Havas, and could count on income from her dowry of 40,000 FF a year. Among his political friends he counted Daniel Wilson, son-in-law of President of the Republic Jules Grévy.⁴² Trouard-Riolle's brilliant prospects were nearly destroyed, however, when, under mysterious circumstances, he fought a duel in Switzerland in August 1890. After reports of this incident appeared in the Paris press, he resigned his office, but only for form's sake. He had arranged with the attorney general of the Cour d'Appel de Paris to be reappointed to his post on the tribunal in November, exactly three months later.⁴³

Trouard-Riolle served five more years with the Tribunal de la Seine before he was promoted to assistant attorney general of the Cour d'Appel de Paris. He had by then gained the patronage of Félix Faure, president of the Republic—some whispered that it was because Faure coveted Trouard-Riolle's wife. The duel was explained away as having occurred "pour des motifs étrangers."⁴⁴ Another advancement came in 1901, this time to the post of solicitor general on the same parquet. In this capacity he prosecuted Mme. Steinheil in 1909. Against the consummate performance of Meg Steinheil from the defendant's rail, any prosecutor would have seemed pallid; yet Trouard-Riolle's summation was particularly weak. Even Victor Fabre, ever solicitous to defend the members of his parquet, could offer only this disingenuous comment: "Whatever has been said of it, and in spite of several imperfections of detail, this summation, which lasted six hours, was fine and eloquent. The case was among the most difficult, having been poorly prepared [a reference to Leydet's work]."⁴⁵ There was one last promotion for Trouard-Riolle, to solicitor general of the Cour de Cassation in 1917. His wealthy wife had died five years earlier, and he celebrated his elevation by marrying for a second time. Despite his position on the highest court, he was never accorded recognition by his fellow magistrates as a conscientious jurist.

The least taxing and most rapid means of achieving high status in the magistrature was through nepotism. Manuel Achille Baudouin (1846–1917) had in his favor a father, Louis Baudouin, who rose to the post of attorney general of the Cour de Cassation, and a father-in-law, Tiengon de Tréferion, who was an associate justice on the Cour d'Appel de Rennes. There was great wealth on both sides of Baudouin's family. The younger Baudouin began as an assistant prosecutor at Châteaulin in 1869, but after seasoning there and at Quimper in 1871, he moved up rapidly. He was an assistant attorney general at Rennes in 1872, a solicitor general on the appellate court at Lyon in 1880, attorney general at Limoges in 1885, and then solicitor general of the Cour de Cassation in 1890, at the age of forty-six. Three years later, he was made chief judge of the Tribunal de la Seine. In 1901, he followed his father as attorney general of the Cour de Cassation, and, in 1911, he was made chief justice of the court.⁴⁶ With such a career, he was easily the most successful magistrate of the first half of the Third Republic.

⁴² *Ibid.*: notations of 1884, 1885.

⁴³ *Ibid.*: documents of August–November 1890.

⁴⁴ *Ibid.*: notations of November 14, 1895, and 1896.

⁴⁵ *Ibid.*: memorandum of December 1909.

⁴⁶ AN, BB 6 II, 650, dos. Manuel Achille Baudouin.

Baudouin's counterpart for the second half of the Republic was Théodore Paul Lescouvé (1864–1940), whose father, Alfred Lescouvé, was an associate justice on the Cour de Cassation, and who had the backing of Louis Barthou. After appointment as assistant prosecutor at Tours and Lyon, Lescouvé was already being hailed as a “magistrate of the future” by 1894, after only four years of service. He solidified his chances with an advantageous marriage to the wealthy Blanche Morand in 1896 and by his clear allegiance to the Republican political cause. The Ministry of Justice moved Lescouvé to Paris permanently in 1896, when it placed him as an undersecretary at the Place Vendôme. From there, he held positions on the parquet of the Tribunal de la Seine and the Cour d'Appel de Paris before returning to the ministry in 1909 to direct first personnel and a year later criminal affairs. His reward was to be made district attorney of the Tribunal de la Seine (1911), attorney general of the Cour d'Appel de Paris (1917), and attorney general of the Cour de Cassation (1920). Finally, in 1928, he reached the summit as chief justice of the Cour de Cassation.⁴⁷

A less successful variant of this nepotism can be seen in the career of Bernard Théodore Médéric de Valles (1853–1929). He had only mediocre success until he married Yvonne de Marcère, whose father, Émile de Marcère, was a senator and had been a close associate of Gambetta and other founders of the Republic.⁴⁸ Thereafter, he moved swiftly from positions on the parquet of provincial tribunals to district attorney at Melun in 1890. He reached Paris in 1894 as an assistant prosecutor and was advanced to *juge d'instruction* on the Tribunal de la Seine in 1897. He had separated from his wife in 1894, and a divorce followed in 1898.⁴⁹ Nevertheless, de Valles's progress was not slowed for long, as he took for his second wife Jeanne Alinhac, whose father was a bureau chief at the Ministry of Justice. One year after this marriage in 1903, de Valles was elevated to the Cour d'Appel de Paris as an associate justice. In this post he presided over the trial of Mme. Steinheil in 1909, and he was one of the few magistrates involved in the case to emerge with his prestige intact.⁵⁰ In 1916, he was made a presiding justice of the appellate court.

Political reliability and social or political connections were not the only means of advancement. Some magistrates of quite marked talent rose through the ranks solely on merit. The most distinguished of these was Louis Marie Joseph Blondel (1851–19??).⁵¹ His father was a mere *avoué* in Blois, his father-in-law a *propriétaire* of middling wealth. But Blondel was endowed with a mellifluous voice that made his oral presentations memorable and with an insistence on thoroughness that further impressed his superiors.⁵² Between 1875 and 1882, he progressed through the parquets of four provincial tribunals. In 1882, he reached the appellate court level with an appointment as assistant attorney general at Douai. Five years later, he was made a solicitor general on that parquet. After fourteen years of seasoning at Douai

⁴⁷ AN, BB 6 II, 1020, dos. Théodore Paul Lescouvé.

⁴⁸ AN, BB 6 II, 1267, dos. Bernard Théodore Médéric de Valles.

⁴⁹ *Ibid.*: notations of 1884, 1887, 1896.

⁵⁰ *Ibid.*: notations of 1903, 1912.

⁵¹ AN, BB 6 II, 679, dos. Louis Marie Joseph Blondel.

⁵² *Ibid.*: notations of 1881, 1887, 1892.

(until 1896), Blondel was moved to Paris, and, once there, he advanced quickly. He was promoted to solicitor general of the appellate court in 1901 and then of the Cour de Cassation in 1907. He ended his career as a presiding justice of the Cour de Cassation.

Another career made through talent belonged to Louis Émile Henri Boucard (1862–19??).⁵³ Unlike Blondel, whose talent lay in the presentation of a case before the court, Boucard's specialty was the investigation of charges placed before the parquet. This ability gained him an early appointment as *juge d'instruction* in Paris in 1894. During the next twenty-three years, he won universal praise as the most perspicacious magistrate of the rank. In both 1906 and 1912, his superiors noted how much more thorough his investigations were compared to those of any other *juge d'instruction* in Paris. The chief judge of the Tribunal de la Seine composed a long memorandum in June 1910 lauding Boucard as a magistrate to whom could be entrusted "the weightiest and most delicate tasks with the certitude that he would complete them with honor while assuring the safeguard of particular interests placed in peril."⁵⁴ Boucard had an opportunity to prove his ability in the brightest light of publicity in 1914. He was charged with the investigation of Henriette Caillaux's assassination of Gaston Calmette. By common consent—even from Calmette's *Le Figaro* staff—Boucard was scrupulously fair and thorough in a case burdened by the gravest of political implications.⁵⁵ His reward came in 1917 with an elevation to associate justice of the Cour d'Appel de Paris. He became a presiding justice in 1925 and moved to the Cour de Cassation as an associate justice in 1929, a year before his retirement.

TO WHAT EXTENT DOES THIS ANECDOTAL MATERIAL from several personnel dossiers represent the promotion patterns for the magistrature as a whole? The court registers for the period contain the record of appointments to the bench and parquet of the appellate courts and the Cour de Cassation, listing for each promotion the surname of the magistrate, his previous position (*qualité ancienne*), his new position (*fonctions*), the date of the promotion decree and of the swearing-in, the reason he left the court (*causes de la cessation des fonctions*), and the surname of his replacement.⁵⁶ From a tabulation of the information on promotion contained in these registers for the Cour de Cassation and the Cour d'Appel de Paris, a clearer picture of promotion patterns within the magistrature emerges (see Tables 2A and 2B).

During the period 1871 to 1914, 519 individual magistrates changed their status one or more times; thus, 1,255 movements of some kind occurred. Since 410 individuals retired, died in office, or were replaced, 845 of these changes in status

⁵³ AN, BB 6 II, 695, dos. Louis Émile Henri Boucard.

⁵⁴ *Ibid.*: notations of 1896, 1906, 1912. For the report specifically referring to Boucard's skillful handling of the controversy over the death of nationalist deputy Gabriel Syveton six years earlier, see *ibid.*: memorandum from Ditte, June 1910.

⁵⁵ *Ibid.*: reports of April 1914. Also see *Le Figaro*, March–May 1914.

⁵⁶ Personnel de la magistrature, 1864–1899, Cour de cassation et cours d'appel, AN, BB 6 524⁴, and Personnel de la magistrature, 1899–1914, Cour de cassation et cours d'appel, *ibid.*, 524⁵.

represent promotions or steps on the way to promotion to these two most important courts in France. Table 2A displays this information in tabular form, showing the positions from which magistrates were promoted. For example, a magistrate who was promoted to assistant attorney general of the Cour d'Appel de Paris was almost invariably assured of further promotion: of the 121 instances of magistrates in that rank, all but 13 were elevated to a still higher rank. And some positions were obvious stepping stones to further promotion: at least 83 of the assistant prosecutors on the Tribunal de la Seine had later promotions to the two highest courts; specifically, for these 83 the next step up was to the post of assistant attorney general on the Cour d'Appel de Paris. Table 2B displays the same information (it is Table 2A turned on its side), showing from what positions the high court posts were filled. For example, of the 4 magistrates elevated to the position of chief justice of the Cour d'Appel de Paris, 2 had just been associate justices of the Cour de Cassation and 1 each had just held the posts of attorney general and associate justice of the Cour d'Appel de Paris.

The information displayed in Table 2A sustains the hypothesis that, although most promotions within the magistrature appear to have been in-step advancements predicated on previous experience and achievement, this responsible pattern was short-circuited for certain individuals whose performance in especially sensitive posts entitled them to extraordinary consideration. The pattern of promotions for magistrates serving on the Cour d'Appel de Paris offers cogent evidence in favor of the first half of this proposition. When assistant attorneys general were advanced, 41 percent (43 of the 105 promoted) moved to the rank of solicitor general on the same parquet, a simple extension of their responsibilities. Similarly, associate justices were raised to presiding justices on the same bench at the rate of 58 percent (42 of the 73 promoted). Solicitors general of the Cour d'Appel de Paris were elevated to the Cour de Cassation to continue in their function in 30 percent of the instances (14 of the 47 promoted). Of the presiding justices of the Cour d'Appel de Paris who were promoted, 86 percent (24 of 28) were advanced to the Cour de Cassation at the rank of associate justice. The significance of these examples is that the magistrates received an appropriate promotion and remained either on the bench or on the parquet, where their seniority and achievement had been earned.

At the same time, there are examples in Table 2A that support the second half of the thesis. Magistrates who served as high officials within the Ministry of Justice, for however short a period, often emerged from the Place Vendôme with exalted dignity. Of the 39 directors of personnel between 1871 and 1914, 9 (23 percent) were immediately raised to associate justice on the Cour d'Appel de Paris; of the 39 directors of civil and criminal affairs, 18 (46 percent) were immediately elevated even higher, to associate justice on the Cour de Cassation. Almost all had entered the ministry with much less rank. More significantly, magistrates who performed acceptably in critical parquet posts were very often rewarded with promotion to high positions on the bench, where their objectivity was open to question. On the Cour d'Appel de Paris, 44 percent of the assistant attorneys general (46 of the 105 promoted) were raised to associate justice; 23 percent of the solicitors general (11 of the 47 promoted) were advanced to presiding justice. Attorneys general were

TABLE 2A
Positions for Senior Judicial Promotion in the Early Third Republic

Promotion																						Total	
Previous Position		Premier Président, Cassation	Procureur Général, Cassation	Président de Chambre, Cassation	Conseiller, Cassation	Avocat Général, Cassation	Premier Président, Paris	Procureur Général, Paris	Président de Chambre, Paris	Conseiller, Paris	Avocat Général, Paris	Substitut Général, Paris	Président, Seine	Procureur de la République, Seine	Premier Président, Provincial	Procureur Général, Provincial	Directeur des Affaires Civiles	Directeur des Affaires Criminelles	Directeur du Personnel	Décédé (Died)	Retraité (Retired)	Remplacé (Replaced)	Total
	Premier Président, CASSATION	—																		1	4	2	7
	Procureur Général, CASSATION	2	—																	4	2	...	8
	Président de Chambre, CASSATION	2	5	—																8	8	...	23
	Conseiller, CASSATION	1	...	15	—		2	4												56	70	6	154
	Avocat Général, CASSATION	4	23	—	...	2					1							4	4	3	41
	Premier Président, PARIS	1	1	...	—	2	...	4
	Procureur Général, PARIS	4	1	...	1	—							2					...	1	1	10
	Président de Chambre, PARIS	24	—				1	1	1	...		1	...	11	15	1	55
	Conseiller, PARIS	1	1	...	1	...	42	—	1	2	9	8	3	3	2	66	112	12	263
	Avocat Général, PARIS	14	...	1	11	7	—	5	1	3	3	1	1	2	1	1	51
	Substitut Général, PARIS	46	43	—	11	1	2	5	4	1	8	121
	Président, SEINE	...	1	...	1	—	2
	Procureur de la République, SEINE	7	2	...	4	—	13

TABLE
Promotions within the Magistrature

Previous Position Promotion	Premier Président, Cassation	Procureur Général, Cassation	Président de Chambre, Cassation	Conseiller, Cassation	Avocat Général, Cassation	Premier Président, Paris	Procureur Général, Paris	Président de Chambre, Paris	Conseiller, Paris	Avocat Général, Paris	Substitut Général, Paris	Président, Seine	Procureur de la République, Seine	Vice-Président, Seine
Premier Président, CASSATION	—	2	2	1
Procureur Général, CASSATION	...	—	5	1
Président de Chambre, CASSATION	—	15	4	1	4	...	1
Conseiller, CASSATION	—	23	1	1	24	1	1	7	...
Avocat Général, CASSATION	—	14	2	...
Premier Président, PARIS	2	...	—	1	...	1
Procureur Général, PARIS	4	2	...	—	1	4	...
Président de Chambre, PARIS	—	42	11
Conseiller, PARIS	—	7	46	96
Avocat Général, PARIS	1	—	43	1
Substitut Général, PARIS	—
Président, SEINE	1	1	—
Procureur de la République, SEINE	1	2	5	—	...
Premier Président, PROVINCIAL	2	1	9	1
Procureur Général, PROVINCIAL	8	3	11
Directeur des Affaires Civiles	3	3	1
Directeur des Affaires Criminelles	1	3	1	2
Directeur du Personnel	2	1	5
Décédé (Died)	1	4	8	56	4	11	66	2	4
Retraité (Retired)	4	2	8	70	4	2	1	15	112	1	1
Remplacé (Replaced)	2	6	3	...	1	1	12	1	8

SOURCE: Archives Nationales, BB 6 524⁴–524⁵.

2B

in the Early Third Republic

<i>Juge d'Instruction, Seine</i>	<i>Juge, Seine</i>	<i>Substitut, Seine</i>	<i>Premier Président, Provincial</i>	<i>Procureur Général, Provincial</i>	<i>Président de Chambre, Provincial</i>	<i>Conseiller, Provincial</i>	<i>Avocat Général, Provincial</i>	<i>Procureur de la République, Provincial</i>	<i>Substitut Général, Provincial</i>	<i>Directeur des Affaires Civiles</i>	<i>Directeur des Affaires Criminelles</i>	<i>Directeur du Personnel</i>	<i>Professeur de Droit</i>	<i>Politician</i>	<i>Avocat</i>	<i>Miscellaneous</i>	<i>Total</i>
...	1	1	...	7
...	1	...	1	1	9
...	25
...	38	15	6	...	1	5	13	...	3	3	3	9	154
...	14	...	1	3	1	...	1	...	3	39
...	4
...	1	1	13
...	1	1	1	1	57
38	2	...	19	20	23	8	3	4	9	2	...	6	13	296
...	1	1	1	...	2	3	3	56
3	...	83	...	4	1	...	11	8	1	1	2	4	118
...	2
...	8
...	—	13
...	—	22
...	—	7
...	—	7
...	—	8
...	156
...	220
...	34

NOTE: This table covers 1,255 changes in status involving 519 individual magistrates.

treated even more handsomely: 13 percent (1 of 8) promoted to chief justice of the court, 63 percent (5 of 8) to the Cour de Cassation (4 to presiding justice, 1 to associate justice). Less conclusive but striking is the fact that 38 *juges d'instruction* from the Tribunal de la Seine were elevated directly to the rank of associate justice of the Cour d'Appel de Paris.

The data in Table 2B substantiate the hypothesis based on Table 2A. A clear relationship frequently exists between the *qualité ancienne* and the new *fonctions*. Of the assistant attorneys general on the Paris parquet, 70 percent (83 of 118) had held the same post on the Tribunal de la Seine. Likewise, magistrates who became solicitors general on the Cour d'Appel de Paris had in 77 percent of the cases (43 of 56) served an apprenticeship for the advance as an assistant attorney general. For positions on the bench, the rule holds true in some instances: of the presiding justices for the Cour d'Appel de Paris and the Cour de Cassation, 74 percent (42 of 57) and 60 percent (15 of 25), respectively, had been associate justices immediately before. But Table 2B also provides information about magistrates who entered the ministry to serve as bureau chiefs and emerged with significant promotions. Most of the directors of personnel moved from assistant attorney general to associate justice on the Cour d'Appel de Paris after a stint in the ministry. For the more important directors of civil and criminal affairs, the jump was considerably greater: from associate justice on the Cour d'Appel de Paris to associate justice on the Cour de Cassation. Only one magistrate during the entire period managed such an advance without an intervening promotion (0.3 percent of the total who were associate justices on the Cour d'Appel de Paris, 0.6 percent of those who became associate justices on the Cour de Cassation). Yet a short service in the ministry, with its exposure to sensitive cases and documents and to the politically powerful, made the elevation routine.

In Table 3, the years 1871 to 1914 are divided into five periods, approximating the political coloration of the first half of the Third Republic, and that division permits seeing the data from Table 2B according to political "ideology" for direct promotion to the two highest courts, the Cour d'Appel de Paris and the Cour de Cassation. From 1871 to 1878, the forces of "Moral Order" held control of the government, and monarchist sentiment was great (much less so, of course, after the failure of the Seize Mai ministry in 1877); the Opportunist Republicans, moderate but clearly Republican liberals, dominated from 1879 to 1894; the relaxing of hostility toward suitably disposed Catholic monarchists and conservative Republicans (the Ralliement) marked the years 1895 to 1898; Radical Republicans held sway from 1899 to 1911; *Poincarisme* and the return of moderate Republicans distinguished the last three years before the First World War, 1912–14. The data in Table 3 can be tabulated to reveal the average number of promotions to these two courts per year for each period (a low of 16 and a high of 19, with a mean of 17.7) and can be broken down to examine whether different promotion policies characterized these five periods.

Table 4 assembles information from Table 3 by political period about promotions to the Cour d'Appel de Paris and the Cour de Cassation that may be considered suspicious: those involving a change from the parquet to the bench

(Type 1), an out-of-step elevation for a *juge d'instruction* (Type 2), and an out-of-step advancement for a bureau chief in the Ministry of Justice (Type 3). Because the periods vary in length from three years to sixteen years, it is impossible to draw any absolutely firm conclusions from these statistics. Nevertheless, it is immediately apparent that, although the years of Opportunist (1879–94) and Radical (1899–1911) rule produced the largest totals, the annual averages of suspicious promotions to the two highest courts, like the average number of total promotions to the same courts, for all five periods are quite similar. The Opportunist Republicans traditionally charged that the judiciary was widely exploited by the governments of “Moral Order” before it came into their hands. Yet the averages for the years 1871 to 1878 and 1879 to 1894 are almost identical. Likewise, Ralliés denounced the “pork barrel” promotions of the Radicals, but the averages for 1895 to 1898 are higher than those for 1899 to 1911. In fact, the blame should be spread equally. None of the five periods can escape the condemnation of these figures: for the first half of the Third Republic, there were 221 “suspicious” promotions to the Cour d’Appel de Paris and the Cour de Cassation alone (roughly 5 per year, regardless of political period). An analysis of the other appellate courts and the most important tribunals, especially the Tribunal de la Seine, would surely reveal many more.

THE PROMOTION POLICIES REVEALED in these statistics and case histories mean that the magistrature of the Third Republic was not, and could not claim to be, an unsullied guardian of right. The probity and impartiality of the judges and prosecutors to whom this ideal was entrusted could not be guaranteed as long as their careers were frequently the instruments of political rancor and reward. But to conclude that French justice was therefore a miasma of corruption is equally erroneous. Despite dramatic lapses, in the majority of cases brought before it, and particularly those in which politics played no role, the magistrature seems to have functioned reasonably well.⁵⁷ The examples of Leydet, Fabre, Albanel, and Monier demonstrate that magistrates could believe so much in their own importance and immunity that they overstepped the bounds, thinking they could get away with it. The examples also suggest that such behavior could lead to their disgrace at what should have been the pinnacle of their careers. In addition, whatever the factional tone of the party in power, almost all of France’s leaders recognized that the 1883 purge had to serve more as a cautionary tale than as a standing threat. The system could not have survived many such shocks without the loss of all public esteem and confidence.

The conduct and procedures of the judicial bureaucracy were, in fact, typical of the pervasive banal hypocrisy of Third Republic France, where style often replaced substance, rhetoric took the place of policy, and the ambiguity of Belle Époque private morality spilled over into public life. The notion of irremovability implied

⁵⁷ See La Correspondance de procureurs généraux avec la garde des sceaux (division criminelle), 1871–1913, AN, BB 18, 1796–2529. If it is possible to generalize about such a mass of documents, there appears to have been an effort at objectivity in nonpolitical cases; there is clear evidence of favoritism and even corruption in the handling of politically sensitive cases.

TABLE 3
Promotions within the Magistrature by Political Period

Promotion	Previous Position	1871-78	1879-94	1895-98	1899-1911	1912-14	1871-1914
<i>Premier Président, CASSATION</i>	<i>Procureur Général, Cassation</i>		1		1		2
	<i>Président de Chambre, Cassation</i>	1			1		2
	<i>Conseiller, Cassation</i>	1					1
	<i>Politician</i>			1			1
	<i>Avocat</i>			1			1
	Total	2	1	2	2	0	7
<i>Procureur Général, CASSATION</i>	<i>Président de Chambre, Cassation</i>	1	4				5
	<i>Président, Seine</i>				1		1
	<i>Professeur de Droit</i>		1				1
	<i>Avocat</i>				1		1
	<i>Miscellaneous</i>	1					1
	Total	2	5	0	2	0	9
<i>Président de Chambre, CASSATION</i>	<i>Conseiller, Cassation</i>	4	6		4	1	15
	<i>Avocat Général, Cassation</i>	2	1		1		4
	<i>Premier Président, Paris</i>		1				1
	<i>Procureur Général, Paris</i>		3		1		4
	<i>Conseiller, Paris</i>					1	1
	Total	6	11	0	6	2	25
<i>Conseiller, CASSATION</i>	<i>Avocat Général, Cassation</i>	5	8	2	5	3	23
	<i>Premier Président, Paris</i>	1					1
	<i>Procureur Général, Paris</i>				1		1
	<i>Président de Chambre, Paris</i>	6	8	3	5	2	24

TABLE 3—continued

Promotion	Previous Position	1871-78	1879-94	1895-98	1899-1911	1912-14	1871-1914
<i>Procureur Général,</i> PARIS	<i>Conseiller, Cassation</i>		2		1	1	4
	<i>Avocat Général, Cassation</i>		1		1		2
	<i>Avocat Général, Paris</i>		1				1
	<i>Procureur de la République, Seine</i>		2		2		4
	<i>Procureur Général, Provincial</i>	1					1
	Politician	—	—	—	—	—	—
	Total	1	7	0	4	1	13
<i>Président de Chambre,</i> PARIS	<i>Conseiller, Paris</i>	8	11	5	14	4	42
	<i>Avocat Général, Paris</i>	2	6	1	1	1	11
	<i>Procureur de la République, Provincial</i>				1		1
	<i>Président de Chambre, Provincial</i>		1				1
	<i>Directeur des Affaires Criminelles</i>				1		1
	<i>Directeur du Personnel</i>	—	—	—	—	—	—
	Total	10	19	6	17	5	57
<i>Conseiller,</i> PARIS	<i>Avocat Général, Paris</i>	1	1	1	4		7
	<i>Substitut Général, Paris</i>	10	14	6	13	3	46
	<i>Vice-Président, Seine</i>	21	33	9	29	4	96
	<i>Juge d'Instruction, Seine</i>	7	13	3	11	4	38
	<i>Juge, Seine</i>				1	1	2
	<i>Premier Président, Provincial</i>	3	5	2	8	1	19
	<i>Procureur Général, Provincial</i>	3	9	3	4	1	20
	<i>Président de Chambre, Provincial</i>	4	9	2	8		23
	<i>Conseiller, Provincial</i>						8
	<i>Avocat Général, Provincial</i>		8				8
			3				3

<i>Procureur de la République, Provincial</i>				1	1	2	4
<i>Directeur du Personnel</i>	1		2	3	3		9
<i>Professeur de Droit</i>			1		1		2
<i>Avocat</i>			3		3		6
Miscellaneous	4		4	1	3	1	13
Total	54		105	31	89	17	296
<hr/>							
<i>Avocat Général,</i>							
PARIS							
<i>Conseiller, Paris</i>					1		1
<i>Substitut Général, Paris</i>	6		22	3	9	3	43
<i>Vice-Président, Seine</i>					1		1
<i>Premier Président, Provincial</i>			1				1
<i>Procureur Général, Provincial</i>			1				1
<i>Président de Chambre, Provincial</i>			1				1
<i>Avocat Général, Provincial</i>	1		1				2
<i>Directeur du Personnel</i>				1	1	1	3
Miscellaneous	2				1		3
Total	9		26	4	13	4	56
<hr/>							
<i>Substitut Général,</i>							
PARIS							
<i>Juge d'Instruction, Seine</i>			2		1		3
<i>Substitut, Seine</i>	15		33	8	21	6	83
<i>Procureur Général, Provincial</i>			1	1	2		4
<i>Président de Chambre, Provincial</i>			1				1
<i>Procureur de la République, Provincial</i>	1		2	1	4		8
<i>Avocat Général, Provincial</i>			7	1	2	1	11
<i>Substitut, Provincial</i>			1				1
<i>Directeur du Personnel</i>			1				1
<i>Avocat</i>			2				2
Miscellaneous			1	1	2		4
Total	16		51	12	32	7	118
<hr/>							
TOTAL	147		299	76	207	49	778

TABLE 4
Out-of-Step and Parquet to Bench Promotions

Type of Change	1871-78		1879-94		1895-98		1899-1911		1912-14		1871-1914	
	Number	Per Year	Number	Per Year	Number	Per Year	Number	Per Year	Number	Per Year	Number	Per Year
TYPE 1	28	3.5	58	3.6	16	4.0	37	2.8	11	3.7	150	3.4
TYPE 2	7	0.9	13	0.8	3	0.8	11	0.8	4	1.3	38	0.9
TYPE 3	6	0.8	12	0.8	4	1.0	10	0.8	1	0.3	33	0.8
TOTAL	41	5.2	83	5.2	23	5.8	58	4.4	16	5.3	221	5.1

TYPE 1 = Promotion from parquet to bench; 2 = Out-of-step promotion for *juge d'instruction*; 3 = Out-of-step promotion for bureau chief.

NOTE: The total number of promotions on which these figures are based is 778. Thus, 28.41 percent of the promotions are "unusual"
—promotions not predicated on previous experience and achievement.

independence, but the chase for promotion assured governmental control; the ideal of a magistrature free from favoritism and politics was honored in the breach. To introduce a three-volume analysis of the French judicial system, Raoul de la Grasserie, a distinguished jurist, wrote in 1914 what he could have written at any point in the nineteenth century: “Le juge doit être indépendant ... au cas contraire, il lui faudra de héroïsme pour être juste.”⁵⁸ Heroism is always a limited quantity. The history of promotion practices within the magistrature during the Third Republic helps illuminate the fundamental problem of the period: Why did the Republic, whose ideals held such promise, decompose into an unstable, lackluster state?

⁵⁸ La Grasserie, *De la justice en France*, 1: 7.

Technocracy and Statecraft in the Space Age —Toward the History of a Saltation

WALTER A. MCDUGALL

THE "SPACE AGE," born with the first artificial earth satellites in the autumn of 1957, is already twenty-five years old. The origins of space technology have passed into contemporary history even as the Space Shuttle, the European rocket *Ariane*, permanent Soviet space stations, and the prospect of space-based laser weapons open a second Space Age of ineffable potential. What is the Space Age? Did *Sputnik I* mark the beginning of a distinct period in the history of human institutions and collective behavior? These questions matter at a moment in history when our societies, politics, economies, and diplomacy are wrenched by perpetual technological revolution.

The *prima facie* case is impressive for marking the technological turning point of the mid-twentieth century at the birth of the Space Age. The first Sputniks seemed to overturn the foundations of the post-World War II international order. They promised imminent Soviet strategic parity, placed the United States under direct military threat for the first time since 1814, triggered a quantum jump in the arms race, and undermined the calculus on which European, Chinese, and neutralist relations with the superpowers had been based. The space and missile challenge was then mediated by massive state-sponsored complexes for research and development, in the United States and throughout the industrial world, into institutionalized technological revolution and, hence, accelerating social, economic, and perhaps cultural change. Space technology altered the very proportions of human power to the natural environment in a way unparalleled since the spread of the railroads. Machines can now travel, deal destruction, store and transmit information, observe and analyze the earth and universe at orders of magnitude beyond what was possible before 1957. Virtually every field of natural science has leapt forward or been transformed on the strength of space-based experimentation and data. Sputnik would seem to qualify as a historical catalyst.¹

I thank the staff of the NASA History Office, especially Monte D. Wright and Alex Roland, for their generous assistance in the research leading to this article and Loren Graham, Melvin Kranzberg, Bruce Mazlish, and Reginald Zelnick for valuable suggestions. I also thank the Committee on Research of the University of California, Berkeley, the Woodrow Wilson International Center for Scholars, and my colleagues at Berkeley for their support and encouragement.

¹ See Robert Lapidus, "Sputnik and Its Repercussions: A Historical Catalyst," *Aerospace Historian*, 17 (1970): 88–93. Walt W. Rostow has also described Sputnik as the turning point in recent history without, however,

Has similar distinctly Space Age change occurred in domestic and international politics? The explosion of science and technology and its impact on society in the 1960s inspired numerous suggestions that this was so. John K. Galbraith and Daniel Bell wrote of the postindustrial age, Charles S. Maier of an age in which intense political exploitation of collective human thought replaced that of raw materials and labor. Even Soviet theorists adopted the phrase "scientific and technological revolution" as a stock description of the post-Sputnik world, in which capitalism itself altered its laws and Leninist doctrine was modified to include science as a "direct productive force."² The four realms most often cited as the loci of revolutionary change in the Space Age are (1) international politics, (2) the political role of science and scientists, (3) the relationship of the state to technological change, and (4) political culture and values in nations of high technology. This essay describes the literature on the impact of space technology in these four realms and proposes some hypotheses for future inquiry.³ Its findings suggest that those who speak of the revolutionary consequences of space and related technologies and those who belittle their impact both exaggerate the reality. For the history of the relationship of politics and technology is evolutionary. Since the 1860s governments have steadily increased their interest in the direct fostering of scientific and technological progress. But within that evolution Sputnik triggered an abrupt discontinuity, a saltation that transformed governments into self-conscious promoters, not just of technological change but of perpetual technological revolution. This change above all defines the Space Age as a historical period and helps explain what most alert undergraduates would attest: that history, in our times, is speeding up.

WHY SPACE FLIGHT IN THE MID-TWENTIETH CENTURY? Although the mathematical, chemical, and metallurgical skills necessary for practical research into rocketry were present by the 1920s, the investment required for orbital flight was so large and the immediate military or economic benefits so uncertain that the genesis of spaceflight in our time is no more self-explanatory than Iberian sponsorship of world navigation is for the fifteenth century. Only in the late nineteenth century did Konstantin Tsiolkovsky and others of his generation first establish that rockets were the practical means to break the chains of gravity and realize the ancient fantasy of voyages beyond the atmosphere. As late as the 1930s rocketry still belonged to determined individuals like Robert Goddard in the United States or amateur clubs like Hermann Oberth's *Verein für*

examining the impact of space-related technologies; Rostow, *The Diffusion of Power: An Essay in Recent History* (New York, 1972).

² Galbraith, *The New Industrial State* (New York, 1971); Bell, *The Coming of Post-Industrial Society* (New York, 1973); and Maier, Introduction to George B. Kistiakowsky, *A Scientist in the White House* (Cambridge, Mass., 1976). On Soviet interpretations of the "scientific-technological revolution," see Bruce Parrot, *Politics and Technology in the Soviet Union* (Cambridge, Mass., in press), chap. 6.

³ The role of technological innovation as a cause of international political change has recently been argued by Daniel K. Headrick; see Headrick, *The Tools of Empire: Technology and European Imperialism in the Nineteenth Century* (Oxford, 1981).



Figure 1: Soviet depiction of Africa saluting the cosmonauts—the “morning of the cosmic era.”
Moscow, 1961.

Raumschiffahrt or Frederikh Tsander's G.I.R.D. in the Soviet Union. The roots of the Space Age are really lodged, therefore, in the late 1930s when a peripatetic collaboration between the rocketeers and national military establishments was inaugurated.⁴

Sociologist William Sims Bainbridge sought to unravel the relative roles of individuals and organizations in the origins of spaceflight, a development he has seen as scarcely inevitable, and even accidental.⁵ He has interpreted rocketry as residing outside "normal science" in the Kuhnian sense, and thus explicable only in terms of "social processes that operate outside the conventional market mechanisms"—that is, a "social movement."⁶ Conventional wisdom held that the amateur rocketeers of the 1920s found their work taken up by military authorities, especially in Nazi Germany, who pushed it forward for political ends. In fact, Bainbridge argues, "the Spaceflight Movement caused the German military to be taken up by the rocket"—the governments of Germany, and later the United States and Soviet Union, were exploited by the space enthusiasts for their own purposes. This provocative thesis emphasizes not only the technical virtuosity but also the manipulative skills of men like Wernher von Braun and Walter Dornberger, who were able to sell the keepers of the public purse on projects like the V-2 and giant Saturn rockets that really involved a misallocation of government investment.⁷ Hence, rockets appeared in a "technologically revolutionary situation, defined as the presence of a rich and incompetent patron beset with problems that might be solved through technological innovation. In such a situation some developments may prosper unnaturally."⁸

There are other conditions besides zealous promotion and foolhardy patronage that make for technological advances. The prototype ballistic missile, the V-2, was also the product of the military and economic restrictions on Germany in

⁴ The collection of articles in *Technology and Culture*, 4 (1963), republished and edited by Eugene Emme as *The History of Rocket Technology* (Detroit, 1964), established rocketry as an important field in the history of technology. Also see R. Cargill Hall, ed., *Essays on the History of Rocketry and Astronautics*, 2 vols. (Washington, 1977); Willy Ley, *Rockets, Missiles, and Space Travel* (New York, 1957; 3d rev. edn., 1961); and Wernher von Braun and Frederick I. Ordway III, *History of Rocketry and Space Travel* (New York, 1966). On the Soviet roots, see Nikolai D. Anoschenko, ed., *A History of Aviation and Cosmonautics*, 5 vols. (Washington, 1977); and Anatoli Blagonravov, *Soviet Rocketry: Some Contributions to Its History* (Springfield, Va., 1966), and *USSR Achievements in Space Research* (Washington, 1969). The NASA Historical Publications series is the best source for program histories; see, for example, Constance M. Green and Milton Lomask, *Vanguard: A History* (Washington, 1970); Loyd S. Swenson, Jr., et al., *This New Ocean: A History of Project Mercury* (Washington, 1966); Barton C. Hacker and James M. Grimwood, *On the Shoulders of Titans: A History of Project Gemini* (Washington, 1977); R. Cargill Hall, *Lunar Impact: A History of Project Ranger* (Washington, 1977); and Homer Newell, *Beyond the Atmosphere: Early Years of Space Science* (Washington, 1980).

⁵ Bainbridge, *The Spaceflight Revolution: A Sociological Study* (New York, 1976).

⁶ The origins of Sputnik illustrate the "chicken and egg" debate on the connection of inventions and environment described by Roger Burlingame in his "Technology: Neglected Clue to Historical Change" and by Lewis Mumford in his "History: Neglected Clue to Technological Change," *Technology and Culture*, 2 (1961): 219–39.

⁷ Bainbridge, *The Spaceflight Revolution*, 4–11. On the cost-effectiveness of the V-2, see *ibid.*, 92–107. Bainbridge relied heavily on biographical data about the leaders of the "spaceflight movement." Recent works include P. T. Astashenkov, *Academician S. P. Korolev: A Biography* (Washington, 1973); I. K. Golovanov, *Sergei Korolev: Apprenticeship of a Space Pioneer*, trans. M. M. Samokhvalov and H. C. Creighton (Moscow, 1975); Milton Lehman, *This High Man—The Life of Robert H. Goddard* (New York, 1963); Esther C. Goddard and G. Edward Pendray, eds., *The Papers of Robert H. Goddard*, 3 vols. (New York, 1970); and Erik Bergaust, *Wernher von Braun* (Washington, 1976).

⁸ Bainbridge, *The Spaceflight Revolution*, 107.

the Versailles Treaty, which encouraged promotion of science and technology as an unhindered means of national growth and, incidentally, failed to ban rocket research. The V-2 also stemmed from the growing interwar cooperation among German science, industry, and the army, from the excellence of German metallurgical, chemical, and electrical firms that pioneered private research and development, from the support of a peculiarly “modernist” Third Reich and its hardware-mad Führer, and from the apparent wartime needs of the beleaguered Nazi empire, especially after its loss of air superiority.⁹ The V-2 then evolved into Vostok and the Jupiter-C, the first space boosters, through the pressures of Cold War, the strategic needs of the Soviet Union, the military-scientific establishments dating from the Second World War, and, not least, the last-minute creation of the atomic bomb, without which ballistic missiles would have remained nuisance weapons unworthy of large investment.

The most telling argument against the “social movement” thesis is Soviet origination of the first satellites. After *Sputnik I*, Americans indulged themselves with the notion that Soviet space spectacles were a fluke, the achievement of captured German scientists, or the result of espionage or individual genius. Deliberate scholarship on the history of Soviet rocketry reveals nothing of the sort. Rather, the first assault on the cosmos quite naturally was launched from the world’s first technocratic state—and spaceflight enthusiasts by no means bamboozled the Kremlin in order to win its material support. The proximate cause of the opening of the Space Age was the competition for better means of delivering nuclear weapons after 1945. Josef Stalin accelerated his atomic bomb program after Hiroshima, and as early as 1947 he ordered the rapid development of intercontinental rockets.¹⁰ But, whatever their urgent need for a counterthreat to American bomber bases encircling their territory, Soviet leaders could not have been midwives to an ICBM in ten years but for Russian rocket expertise rooted in the 1920s and the extraordinary budgetary and political force-feeding of research and development dating from 1918.

⁹ On the accomplishments of the Peenemünde team and its escape to the United States, see the magnificent narrative of Frederick I. Ordway III and Mitchell R. Sharpe, *The Rocket Team* (New York, 1979), which is based on extensive research in published, manuscript, and oral history sources.

¹⁰ See G. A. Tokady (Tokaty-Tokaev) in Emme, *History of Rocket Technology*, 271–84. General works on the Soviet space program include Martin Caidin, *Red Star in Space* (n.p., Crowell-Collier Press, 1963), an alarmist tract from the space race; Nicholas Daniloff, *The Kremlin and the Cosmos* (New York, 1972), a well-researched and balanced popular history; James Oberg, *Red Star in Orbit* (New York, 1981), a bold attempt to penetrate official secrecy and disinformation based on a lifetime of space “Kremlinology”; Evgeny Riabchikov, *Russians in Space*, trans. Guy Daniels (New York, 1971), a glorified account by the Novosti Press Agency, but containing new information; Charles S. Sheldon II, *U.S. and Soviet Progress in Space: Summary Data through 1973 and a Forward Look* (Washington, 1974), a source book based on the meticulous data-gathering of the (now deceased) expert of the Congressional Research Service; William Shelton, *Soviet Space Exploration: The First Decade* (New York, 1968), a mission and hardware summary; G. A. Skuridin, ed., *Mastery of Outer Space in the U.S.S.R., 1957–1967* (Washington, 1975), a NSF-NASA collection of Tass releases; Peter L. Smolders, *Soviets in Space*, trans. Marion Powell (New York, 1973), an “objective” narrative by a Dutchman; Michael Stoiko, *Soviet Rocketry: Past, Present, and Future* (New York, 1970), a standard survey; and Leonid Vladimirov, *The Russian Space Bluff* (London, 1971), an intriguing exposé by an emigre who has claimed that early Soviet space spectacles, ordered by Khrushchev for political propaganda and executed only thanks to Korolev’s genius, concealed the true inferiority of Soviet high technology. On Soviet nuclear policy, see Arnold Kramish, *Atomic Energy in the Soviet Union* (Stanford, 1960); and David Holloway, “Entering the Nuclear Arms Race: The Soviet Decision to Build the Atomic Bomb, 1939–45,” International Security Studies Program, Woodrow Wilson International Center for Scholars, July 1979.

The First World War never ended in Russia. There alone the methods of state mobilization of scientific and technical talent pioneered in the first total war became a centerpiece of peacetime policy. Research and development, centralized and liberally funded, amounted to a virtual “second party program,” as Lenin said upon inaugurating the Gosplan in 1920. Technological research was the major tool in the drive to industrialize, to overtake the West, and to prove the superiority of socialism. Spaceflight itself, aside from its military uses, appealed to Lenin¹¹ and, after 1933, to the Politburo, which began to organize and fund rocket research about the same time the German army absorbed its native rocketeers. The nature of the Bolshevik seizure of power and its Communist ideology inevitably placed a premium on the rapid creation of advanced technology as a symbol of the regime’s legitimacy. While ignoring the purges of the 1930s Soviet media exalted the feats of record-breaking aviators—Stalin’s Eagles—as indicative of the “New Soviet Man” and his soaring technology. As late as 1941, meanwhile, Soviet rocketeers were still the theoretical equals of their German counterparts at Peenemünde. But, while the war accelerated German rocketry, it diverted Soviet efforts for the duration. The capture of V-2 production facilities in 1945 afforded Soviet engineers the rapid practical experience they had been denied, but it by no means created their rocket program. An assault on the heavens remained uniquely suited to the Communist scientific conception of the purposeful, self-confident conquest of nature. When wedded to their military requirements after the war, the inherent Soviet interest in spaceflight made Sputnik only a matter of time.¹²

Certain other characteristics of the Soviet regime undoubtedly impeded the advance of rocket technology. The brilliant chief designer of Soviet spacecraft, Sergei Korolev, and the leading expert on fuels and gas dynamics, Valentin Glushko, spent up to eight years in *sharagas*, prison design bureaus, during the purges after 1937. Countless other technicians had their careers diverted or cut short by Stalinist terror. But the familiar defects of Soviet science—political interference, terror, incompetence in high places, and secrecy—affected high-priority, military-related research far less than that in civilian spheres. Kendall E. Bailes’s excellent study of Soviet technology before 1941 lists the salient features of research and development: tension between borrowing abroad and pushing native creativity; lack of internal competitive stimuli; inhibition caused by state terror; resistance to creation of a privileged class resulting from professionalization of research; shortage of skilled workers; traditional Russian preference for pure over applied science; isolation of research and development from production; and the tendency to place technical considerations over economic efficiency.¹³ Added to the inherited backwardness of the Russian economy, these features of the Communist system suggest that the precocious leap into space was an anomaly. But military technology was a special case in every respect. It enjoyed a

¹¹ Daniloff, *The Kremlin and the Cosmos*, 22.

¹² This thesis is discussed in more detail in Chapter I, “The Genesis of Sputnik,” of my book in preparation, a political history of the first decade of space technology.

¹³ Bailes, *Technology and Society under Lenin and Stalin: Origins of the Soviet Technical Intelligentsia, 1917–1941* (Princeton, 1978), 341–42.

priority of talent, funds, and materiel. Close political supervision, harmful in other sectors, aided strategic technology by eliminating bottlenecks. Most of the negative characteristics listed by Bailes did not apply to rocketry, while the regime's political need to demonstrate the military and technological superiority of the Socialist Fatherland was a strong fillip. Given the native Russian talent, the assist (though minor) provided by Helmut Gröttrup's residual German team, and the full support of a peacetime command economy, the Soviet leap into space becomes less mysterious.¹⁴ What still remains to be uncovered, however, is the relationship between high-technology industries (many of which must be integrated in rocketry) and the sophistication of an economy as a whole. The debate over the role, or indeed the origin, of "leading sectors" in the English Industrial Revolution, for instance, has implications for the origins of the Space Age.

THE CENTRAL PROBLEM OF EARLY SPACE HISTORY lies in assessing the worldwide impact of *Sputnik I*.¹⁵ The Soviets successfully tested an ICBM in August 1957, but it was the subsequent launching of the first satellite on the shoulders of that great rocket that upset the preconceptions of Americans, Europeans, and Third World elites. How could the United States, on whose superiority Free World strategies depended, have lost the race into space? The post-Sputnik panic, sustained by a veritable "media riot," yielded two contradictory sets of explanations. Senator Lyndon B. Johnson's Senate Armed Services Subcommittee on Preparedness publicized specific explanations based on Republican mismanagement: interservice rivalry, the antipathy to missiles of the "big bomber boys," and Dwight David Eisenhower's stringent budget ceilings all contributed to the Pentagon's "missile mess."¹⁶ In the country at large, pundits, politicians, and

¹⁴ See Bailes, *Technology and Society under Lenin and Stalin*, esp. chaps. 8–9, Vladimirov, however, regarded the space program as a Russian triumph achieved in spite of the Communist rulers; *The Russian Space Bluff*, 164–74. On science in the Soviet Union, also see D. Joravsky, *Soviet Marxism and Natural Science, 1917–1932* (New York, 1961); Loren R. Graham, *The Soviet Academy of Sciences and the Communist Party, 1927–1932* (Princeton, 1967); Mose L. Harvey et al., *Science and Technology as an Instrument of Soviet Policy* (Miami, 1972); and Zhores Medvedev, *Soviet Science* (New York, 1978). The importance of 1932 lay in the "Gleichschaltung" of the Academy of Sciences by the Communist party and the end of independent organized research in the Soviet Union.

¹⁵ On war as a stimulant to technological change, compare John U. Nef, *War and Human Progress* (Cambridge, Mass., 1950), and Walt W. Rostow, "War and Economic Change: The British Experience," in his *The Process of Economic Growth* (2d edn., Oxford, 1960), 144–67. Rostow has argued for a small impact of war on industrialization. For important correctives, see Samuel E. Finer, "State- and Nation-Building in Europe: The Role of the Military," in Charles Tilly, ed., *The Formation of National States in Western Europe* (Princeton, 1975), 84–163; and William H. McNeill, *The Pursuit of Power: Technology, Armed Forces, and Society since A.D. 1000* (in press). On international rivalry and the rapid development of space technology, see William Schauer, *The Politics of Space: A Comparison of the Soviet and American Space Programs* (New York, 1976); and Alain Dupas, *La Lutte pour l'espace* (Paris, 1977). The former is comprehensive but indifferently researched; the latter covers the same ground but suffers from a confessed antisuperpower perspective.

¹⁶ The most thorough research to date is in Edmund Beard, *Developing the ICBM: A Study in Bureaucratic Politics* (New York, 1976). Also see Michael H. Armacost, *The Politics of Weapons Innovation: The Thor-Jupiter Controversy* (New York, 1969); Edgar M. Bortome, *The Missile Gap: A Study in the Formulation of Military and Political Policy* (Cranberry, N.J., 1971); J. L. Chapman, *Atlas: The Story of a Missile* (New York, 1960); and Herbert York, *Race to Oblivion: A Participant's View of the Arms Race* (New York, 1970). Contemporary critiques from irate generals include James Gavin, *War and Peace in the Space Age* (New York, 1958); John B. Medaris, *Countdown for Decision* (New York, 1960); and Maxwell Taylor, *The Uncertain Trumpet* (New York, 1959).

special pleaders portrayed Sputnik as a symbol of a general American malaise: flabby education, denigration of “egghead” scientists, complacency, and consumerism demanded a national soul-searching. LIFE magazine “argued the case for being panicky,” and Bernard Baruch prophesied, “If America ever crashes, it will be in a two-tone convertible.” But historians must avoid reading back into the 1940s and 1950s the assumptions of the Space Age itself. The Truman administration had cancelled the first satellite and ICBM programs begun in 1945 by the Naval Research Laboratory and the Air Force, because no cost-effective mission for large rockets existed until the building of compact, light hydrogen bombs after 1954. Even then, Nelson Rockefeller was almost alone in warning that the prestige value of the first satellite “makes this a race that we cannot afford to lose.”¹⁷ In those years Europe and East Asia were on the front lines of the Cold War, the Red Army and Communist subversion the main threats, the Strategic Air Command and the Central Intelligence Agency the requisite deterrents. A prestige race for “hearts and minds” in a barely emergent Third World in which preeminence in space was to play a major role was inconceivable to Eisenhower before *Sputnik I* and financially reckless thereafter.

Meanwhile, the impending Soviet missile capability, watched closely by the White House throughout the 1950s, suggested not panic but disarmament talks.¹⁸ The Soviet “coming of age,” with its implied threat to the United States, was an apt moment for international control of weapons threatening the entire planet. But far from altering forever the ebb and flow of rivalry and balance of power, the new technologies only reinforced political factors to stymie such control. The tempting utility of thermonuclear missile forces in surprise attack, the long lead times of complex missile and defense systems, the possibility of “technological surprise” for purposes of political blackmail, and the insurmountable problems of verification of disarmament—all weighed against a diplomatic formula for arms control.¹⁹ The absence of diplomatic solutions was a boon to space technology, which would have encountered an ironic hurdle if “outer space missiles,” as they were called, had been banned or severely controlled at the outset. Other strategic imperatives favored rapid development of space technology regardless of presidential will or world view. First, the simple fact that the Cold War pitted an open society against a closed one placed a premium on surreptitious surveillance techniques for the United States, whether an arms

¹⁷ On early American satellite proposals, see R. Cargill Hall, “Earth Satellites: A First Look by the U.S. Navy,” paper presented at the Fourth History Symposium of the International Academy of Astronautics, held in October 1970; and RAND SM-11827, “Preliminary Design of an Experimental World-Circling Spaceship,” May 2, 1946. Rockefeller’s comments were attached to NSC 5520, “Satellite Program,” May 20, 1955, Dwight David Eisenhower Library, Abilene, Kans.

¹⁸ The American intelligence community estimated in January 1955 that the Soviet Union would have operational intercontinental ballistic missiles by 1963–1960 at the earliest; the estimates, though not far wrong, ignored the political impact of the earlier test rockets, such as the ones that launched the Sputniks. “Basic National Security Policy,” January 7, 1955, Eisenhower Library, NSC 5501.

¹⁹ Eisenhower had called for United Nations control of “outer space missiles” in January 1957 as well as “Open Skies” for monitoring disarmament in 1955, and he began negotiations for a nuclear test ban in 1958. See James Killian, *Sputnik, Scientists, and Eisenhower: A Memoir of the first Special Assistant to the President for Science and Technology* (Cambridge, Mass., 1977); and Robert A. Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954–1960* (London, 1978).



Figure 2: An impressive rocket launch from Cape Canaveral. The Air Force *Midas II*, aboard an Atlas-Agena rocket, was successfully placed in orbit on May 24, 1960—the first American surveillance (“spy”) satellite. Official photograph, reproduced courtesy of the U.S. Air Force.

race or arms control obtained. Contracts for the development of spy satellites had already been let in 1956, before *Sputnik I*, and there is evidence that concern for the establishment of the legality of satellite overflight (or “freedom of space”) figured in Eisenhower’s insistence on the civilian Vanguard satellite program and increased risk of losing the satellite “race.”²⁰ A Soviet strategic capability made an American push for a sophisticated and invulnerable surveillance technique—that is, unlike the U-2—inevitable. Second, an ICBM force adequate for any strategy beyond the crudest “city-busting” deterrent required supporting satellite systems for geodesy, meteorology, targeting, infrared early warning, electronic ferreting, and surveillance.²¹ The Soviet Union rebuffed American demarches for control of missile and space technology with their habitual demands for removal of foreign bases and “general and complete disarmament” without inspection, while the United States carefully promoted a ban on “aggressive” rather than “military” uses of space, in order to shelter its own military satellites. Thus, while the only objects in orbit—Sputniks, Explorers, and Vanguards—were still contributions to the International Geophysical Year, and hopeful globalists cried “Space for Peace,” the militarization of space proceeded apace.²²

The Soviets were as surprised as anyone by the impact of their Sputniks and Luniks. Their propaganda value contributed to great changes in the Soviet Union, including another jolt upward in research and development expenditures. Nikita Khrushchev initiated his own “New Look” defense policy by sacking the traditionalist war hero Marshal G. K. Zhukov just after *Sputnik I* and giving priority to the new strategic rocket forces.²³ In the United States, the “missile gap” furor helped elect John Kennedy to the presidency and kicked off an arms build-up on the American side (especially the thousand Minuteman ICBMs and

²⁰ Donald Quarles, Deputy Secretary of Defense, in Cabinet Minutes, October 18, 1957, Eisenhower Library; and Dwight D. Eisenhower, *The White House Years*, volume 2: *Waging Peace, 1956–1961* (New York, 1965), 210.

²¹ These simple facts about the passive militarization of space escaped the understanding of most journalists, politicians, and citizens for years. I found the most succinct early statement of the complementarity of long-range missiles and military satellite systems in Colonel Petkovsek’s “L’Utilisation militaire des engins spatiaux,” *Revue militaire générale* (July 1961), following the ideas of General Pierre Gaultois. Unlike American leaders, de Gaulle felt it advantageous to popularize, not downplay, the military space effort.

²² American policy for outer space, including tactics for international legal protection of military satellites, was codified in NSC 5841/1, “Preliminary US Policy on Outer Space,” August 18, 1958, and NSC 5918, “US Policy on Outer Space,” December 17, 1959. On the politics of “spy satellites,” see Gerald M. Steinberg, “The Legitimization of Reconnaissance Satellites: An Example of Informal Arms Control” (Ph.D. dissertation, Cornell University, 1981); and see Philip Klass, *Secret Sentries in Space* (New York, 1971). John Taylor and David Monday’s *Spies in the Sky* (New York, 1973) only concerns air-breathing spy planes. Surveys of military space developments include Eldon W. Downs, *The U.S. Air Force in Space* (New York, 1966); Michael N. Golovine, *Conflict in Space: A Pattern of War in a New Dimension* (London, 1962); Robert Salkeld, *War and Space* (Englewood Cliffs, N.J., 1970); and Bhupendra M. Jasani, *Space—Battlefield of the Future?* (Stockholm, 1978). Military space systems can only increase in importance as the United States and Soviet Union move toward operational antisatellite weapons and possibly space-based lasers.

²³ See Herbert S. Dinerstein, *War and the Soviet Union: Nuclear Weapons and the Revolution in Soviet Military and Political Thinking* (New York, 1962); Marshal Sokolovskii et al., *Soviet Military Strategy* (“Voennaia Strategiia”), RAND R-416-PR (1963); Roman Kolkowicz, *The Soviet Military and the Communist Party* (Princeton, 1967); and *The Impact of Technology on the Soviet Military: A Challenge to Traditional Military Professionalism*, RAND RM-4198-PR (1964). On early debate over the military uses of space, see Herbert L. Sawyer, “The Soviet Space Controversy, 1961 to 1963” (Ph.D. dissertation, Fletcher School of Law and Diplomacy, 1969).

Polaris submarines) that created a real missile gap in the Soviet Union and, hence, the effort to recoup through placing medium-range missiles in Cuba. Following the October 1962 Cuban crisis, the two superpowers moved quickly to a Partial Test Ban Treaty, after which the Arms Control and Disarmament Agency revived hopes for a freeze on missile technology before antiballistic missiles, multiple independently targeted re-entry vehicles (MIRVs), increased accuracy, and other "improvements" destabilized the balance of terror. NASA reported with alarm that fully eleven of fourteen anticipated missile improvements the arms controllers hoped to ban were important or vital to the exploration and use of outer space. Control of weapons research (as opposed to deployment) would have strangled space programs in their cradles!²⁴

The problem of technological change for arms control is not merely one of preventing the military application of new technologies; rather, it often lies in the fact that operational systems for civilian and military use are virtually identical. Nevertheless, there was great hope in the early years of space technology that international agreements might preempt militarization of space and otherwise establish international law for behavior in space. The arcane field of space law burgeoned to promote and interpret a number of limited agreements of space diplomacy (United Nations Outer Space Treaty of 1967, conventions on spacecraft registration, liability, astronaut rescue, communications satellites and radio frequencies, and pending treaties on direct broadcast satellites, remote sensing of the earth, and the exploitation of the moon).²⁵ The two early schools of space law, the natural and the positivist, debated the wisdom of establishing codes of behavior a priori for activities in space, as opposed to letting space law, like common law on earth, evolve according to patterns of use and interest.²⁶ The debate hardly seemed an idle one as scholars and statesmen groped for ways to avoid a repetition of the failure to regulate the use of atomic energy after 1945. But technological revolution, as opposed to technology per se, renders law and regulation continually obsolete. International legal commit-

²⁴ This suggestion is derived from research in NASA, ACDA, and other materials. Heretofore, space arms control has consisted of the restrictions in the Limited Nuclear Test Ban Treaty, the Outer Space Treaty, and the ABM (SALT-1) Treaty. Signatories have agreed to prohibit nuclear explosions or deployment of weapons of mass destruction in outer space and any interference with each other's "national means of verification" (spy satellites). See Schauer, *The Politics of Space*, chap. 9; Dupas, *Lutte pour l'espace*, chap. 6; Alton Frye, *Space Arms Control: Trends, Concepts, Prospects*, RAND P-2873 (1964); and Walter C. Clemens, *Outer Space and Arms Control* (Cambridge, Mass., 1966).

²⁵ For useful guides to the literature, see Irvin L. White, *Law and Politics in Outer Space: A Bibliography* (Tucson, 1972); and Kuo Lee Li, *World Wide Space Law Bibliography* (Toronto, 1978). Also see the *Yearbooks of the Institute of Air and Space Law*, McGill University; and United States Senate, Committee on Aeronautical and Space Sciences, *Space Law: Selected Basic Documents* (Washington, 1976).

²⁶ Early classics on space law include the "positivist" Myres S. McDougall's *Law and Public Order in Space* (New Haven, 1963) and the "naturalist" Andrew J. Haley's *Space Law and Government* (New York, 1963). Another seminal work surveys historical examples of international and nonterritorial legal regimes; see Philip C. Jessup and Howard J. Taubenfeld, *Controls for Outer Space and the Antarctic Analogy* (New York, 1959). Other leading expositions of theory for space law include John Cobb Cooper, *Explorations in Aerospace Law* (Montreal, 1968); Stephen Gorove, *Studies in Space Law: Its Challenges and Prospects* (The Hague, 1977); and C. Wilfred Jenks, *Space Law* (New York, 1965). For an Indian view that cites Third World perspectives, see S. Bhatt, *Legal Controls for Outer Space: Law, Freedom, and Responsibility* (New Delhi, 1973). On Soviet policy and jurisprudence for space, see A. S. Piradov, *International Space Law* (Santa Barbara, 1974); and E. G. Vasilevskaya, *Legal Problems of the Conquest of the Moon and Planets* (Santa Barbara, 1974).

tees move slowly; space technology very quickly. Hence, increasingly prolific technologies, far from engendering more complex laws, have revived the notion of law as principle—a “revolutionary” jurisprudence in which the spirit, not the letter, is of the essence.

The Space Treaty banned national claims on celestial bodies and the orbiting of weapons of mass destruction. It also established free access to space for all nonaggressive purposes. Otherwise, technological and geopolitical exigencies have suggested an essentially laissez faire regime for space. After initial Cold War skirmishing, American and Soviet leaders recognized a common interest in preventing undue United Nations constraints. American officials inside and outside the Pentagon expressed a thesis not unlike that of Sir Eyre Crowe's Foreign Office memorandum of 1907 *re* the German dreadnought challenge. Any nation, Crowe wrote, would prefer to rule the waves itself but, failing that, would rather Britain did so. Similarly, U.S. hegemony would safely uphold the freedom of space for all. Soviet literature on strategic doctrine suggests a different analogy (itself first proposed by Lyndon Johnson): as the Roman empire dominated the land by its road system, the British empire the seas, and the American empire the air, so now was outer space the decisive medium; whoever ruled it could dictate events on earth as well.²⁷ Neither superpower showed an interest in the kind of multilateral controls proposed at the United Nations. The only path toward detente in outer space was cooperation among sovereigns. As political scientist Don E. Kash noted in the 1960s, cooperation was a “God word, who could be against it?” Yet the sensitivity of the technology for national military and economic interests circumscribed the possibilities for cooperation among the United States, the Soviet Union, and the Europeans.²⁸ INTELSAT, the global consortium for communications satellites founded in 1963, suggested to some that a technological determinism might substitute for political will in forcing cooperation. By dint of cost and function, this and other economic satellite systems (for example, earth resources surveyors) required functional organizations transcending politics. But experience with international cooperation in space (or the deep sea bed) has not sustained the functionalist hypothesis. INTELSAT was a house of discord until American domination ended in 1971, and the international problem-solving that has occurred did not spill over into other arenas of diplomacy.²⁹

The problems in INTELSAT were in part a phenomenon of uneven growth. True cooperation is impossible when one state has a monopoly of technological

²⁷ Documents in the Lyndon B. Johnson Library reveal that Johnson borrowed this geopolitical synopsis from his aide George Reedy. On Soviet perceptions of the strategic significance of space, see Sawyer, “The Soviet Space Controversy”; and other works cited in note 23, above.

²⁸ On United States policy for space cooperation in the early years, see Arnold Frutkin (chief of NASA office for international affairs), *International Cooperation in Space* (Englewood Cliffs, N.J., 1965); United States Senate, Committee on Aeronautical and Space Sciences, *International Cooperation for Outer Space*, ed. Eilene Galloway (Washington, 1965). Early critics of American “conservatism” in cooperation include Leonard E. Schwartz, “When Is International Space Cooperation International?” *Bulletin of the Atomic Scientists* (June 1963), 12–18, and other articles; and Kash, *The Politics of Space Cooperation* (West Lafayette, Ind., 1967), 10.

²⁹ On INTELSAT, see Jonathon Galloway, *The Politics and Technology of Satellite Communications* (Lexington, Mass., 1972); Judith F. Kildow, *INTELSAT: Policy-Makers' Dilemma* (Lexington, Mass., 1973); and Michael Kinsley, *Outer Space and Inner Sanctums: Government, Business, and Satellite Communications* (New York, 1976).

know-how. But an equitable international division of labor is definable only in political terms. American corporations also proved to be something other than promoters of progress, seeking at times to inhibit exploitation of satellite technology that competed with their oceanic cables. Space applications in general have sparked vigorous European and Japanese rivalry with American space industries, as much for political as economic reasons. And, rather than joining a global communications network, the Soviets established their own INTERKOSMOS system for the Eastern Bloc countries. This, and perfunctory visits by guest cosmonauts to Salyut space stations, hardly constituted genuine sharing of technology. The Europeans, in turn, have had uneven success with their international space agencies.³⁰ Participants often viewed cooperative programs as a means of hastening national technological *independence*, as the French and Japanese cases illustrate.³¹ Even as advanced technology united the world in some respects, the financial, military, and organizational demands of “big science” tended to reinforce the national state as the most efficient *agent* of technological change.

Given this history we may well ask why statesmen and pundits of the early Space Age expected the conquest of space to alter the traditional behavior of states. Perhaps the technological enthusiasm of the 1950s and 1960s had an element of self-justification. Hiroshima and the subsequent absorption of nuclear weapons into the international order were technological *faits accomplis*. But after *Sputnik I* statesmen again proved unable or unwilling to control the accelerating advance of technology; instead they groped for formulas in which technology itself would do the work of the human agency—that is, fashion prophylactics against its own misuse. They professed to see in space exploitation or mutual assured destruction or some other effluence of rocketry an integrative force that would solve its own political problems *en passant*. The evidence suggests rather that nothing in the technology necessarily drew countries together. The poor record of the Atoms for Peace program and later of the International Atomic Energy Agency understandably inclined the pivotal United States toward a conservative policy on space cooperation.³² The Soviet Union has shown little interest in open sharing at all, and superpower cooperation—most notably, the 1975 Apollo-Soyuz rendezvous—has been the result, not the cause, of political detente.³³

³⁰ The European Launch Development Organization (ELDO) and European Space Research Organization (ESRO), founded in the early 1960s to ensure a role for European governments and business in space, were models of how *not* to promote international research and development. Recently, the formation of the European Space Agency (1975) has given European space cooperation a new lease on life, but only by acting as an umbrella for nationally managed programs.

³¹ Walter A. McDougall, “The Struggle for Space,” *Wilson Quarterly*, 4 (1980): 66, 71–82. On European space programs, see note 56, below; and United States House of Representatives, Committee on Science and Technology, *World Wide Space Programs* (Washington, 1977).

³² Frutkin, *International Cooperation in Space*, 28–35.

³³ Writers of the 1970s regarded the space race, like the Cold War, as a thing of the past; see, for instance, J. C. D. Blaine, *The End of an Era in Space Exploration: From Competition to Cooperation* (San Diego, 1976). Edward C. and Linda N. Ezell’s *The Partnership: A History of the Apollo-Soyuz Test Project* (Washington, 1978) includes an excellent summary of the efforts to establish cooperation with the Soviet Union in space and of the technical difficulties of the joint manned mission. As an official history, it does not attempt to evaluate the charge that the ASTP was a “giveaway” of American expertise. Dodd L. Harvey and Linda Ciccoretti’s *U.S.-Soviet Cooperation in Space* (Miami, 1974) is a thorough political history benefiting from access to the papers of NASA administrator James E. Webb.

"The horror of the twentieth century," wrote Norman Mailer, "was the size of each new event, and the paucity of its reverberation."³⁴ *Sputnik I* was truly the shot heard 'round the world, and its international effects were manifold; but it did not alter the nature of the international system. The national state remained supreme, cooperation remained a muted form of competition, and military rivalry incorporated the strategic canopy of orbital space. The international imperative stimulated the rapid development of space technology, but it was not in turn transformed by it. When President Johnson, for instance, sent to world heads of state in December 1968 the famous photograph from *Apollo 8* of the gorgeous blue earth rising beyond the rim of the moon, "There came a response from Hanoi, from Ho Chi Minh, thanking me." Surely, wrote Arthur C. Clarke, this was the best example "of the way space can put our present tribal squabbles in their true perspective."³⁵ Perhaps—but the war continued unabated.

DISCUSSING SCIENCE AND POLITICS, Bertrand de Jouvenel wrote of three ages of history: the age dominated by priests, that by lawyers, and that by scientists. The politics of the first age were based on divine revelation and a presumption of popular ignorance, and the politics of the second on "human scripture" and the presumption that "We the people" were capable of judging matters of common interest; the politics of the third age form an anomaly. *Demos* still has the responsibility for public decisions but has lost the competence to judge matters of science and technology. "This great age of science is, by way of corollary, an age of personal ignorance."³⁶ At some point in this century, advanced societies crossed the line into awareness of democratic incompetence. For the industrial West, *Sputnik* may have been that point. Did the perplexing, frightening, and apparently sudden appearance of space technology humble the West (and perhaps the Politburo) into a reappraisal of traditional management of power by politicians and interest groups? Is the Space Age a time in which control of public policy must fall by default to a technical elite? This prospect obsessed Mao-Tse-Tung as it had Stalin; it also came to trouble Eisenhower.

The apparent solution to Jouvenel's dilemma after *Sputnik* was to graft scientific advice onto the existing political corpus, as if science could inform policy without politics informing science. But "where knowledge is power, the pursuit of knowledge is clearly a political activity."³⁷ Throughout the 1960s scientists and political scientists discussed the relationship of science and government.³⁸ The considerable

³⁴ Mailer, *Of a Fire on the Moon* (New York, 1969), 34.

³⁵ Clarke, *Report From Planet Three* (New York, 1972), 164–65.

³⁶ Jouvenel, "The Political Consequences of the Rise of Science," *Bulletin of the Atomic Scientists* (December 1963), 2–8.

³⁷ Howard J. Taubenfeld, ed., *Space and Society* (Dobbs Ferry, N.Y., 1964), 46. Illustrating the inability of voters to make technical judgments, Taubenfeld cited a straw poll that asked people what they demanded of federal spending for research and development. Three out of five answered "Don't know" or even "Don't understand what you mean."

³⁸ See especially Harvey Brooks, *The Government of Science* (Cambridge, Mass., 1968); Joseph S. Dupre and Sanford Lakoff, *Science and the Nation, Policy and Politics* (Englewood Cliffs, N.J., 1962); A. Hunter Dupree, *Science in the Federal Government* (Cambridge, Mass., 1957); Sanford Lakoff, *Knowledge and Power: Essays on*

role of the President's Science Advisory Committee in the organization of the National Aeronautics and Space Administration (NASA) has also been subject to historical treatment.³⁹ It was in these years when the ideal of "good government" informed by "good science" seems to have been best approximated, at least according to the memoirs of the first two presidential science advisors, James Killian and George Kistiakowsky.⁴⁰ Leaning on his scientists for support in the effort to quell overreaction to Sputnik, Eisenhower oversaw the establishment of a space policy that emphasized science and defense as opposed to engineering showmanship and prestige. But he had to struggle against military and congressional leaders, the aerospace industry, and the press, all of whom exaggerated Soviet capabilities to justify ever greater budgets for research and development. Despite his affection for "my scientists," who were "one of the few groups . . . in Washington who seemed to be there to help the country and not help themselves,"⁴¹ Eisenhower left office fearful of the impact that a headlong technological race with the Soviets would have on American society. He warned against the acquisition of undue power and influence not only by the "military-industrial complex" but also by a "scientific-technological elite."

By the late 1960s, critics of American policies in technology and defense recalled that "Ike tried to warn us." It marked the advent of an Eisenhower revisionism that is still cresting. But a close reading of the Farewell Address reveals that Eisenhower saw the trends he deplored as inevitable, and that he had no remedy for them. In the late 1950s, he had already encountered the quandary of sharply conflicting scientific advice, as well as the need to overrule even unanimous scientific advice for political reasons. Scientists solidly opposed expensive space missions for prestige purposes, especially manned spaceflight. Yet Eisenhower transferred the manned space mission and the Von Braun rocket team to NASA and personally ordered accelerated development of the giant Saturn booster, for which no military or scientific mission existed. Although he backed scientific opposition to the proposed Apollo moon program, it was the cost that appalled him. He had reluctantly granted the need to compete for reasons of prestige.

Eisenhower's fears were confirmed under the succeeding administrations, but not perhaps in the way he expected. In the 1960s, science and technology penetrated numerous corners of government, while enthusiasm spread for technological fixes abroad (whether in military spending or developmental aid) and social engineering at home. Yet the influence of the scientists themselves faded. Why should the progressive and even technocratic administrations of Kennedy and Johnson have reduced the influence of scientific advisors even as they heavily subsidized the research community? Even NASA ceased to be an agency shaped

Science and Government (New York, 1971); Ralph E. Lapp, *The New Priesthood* (New York, 1965); and Don K. Price, *The Scientific Estate* (Cambridge, Mass., 1965).

³⁹ See Richard Hirsch and Joseph Trento, *The National Aeronautics and Space Administration* (New York, 1973); Arthur L. Levine, *The Future of the U.S. Space Program* (New York, 1975); and Robert L. Rosholt, *The Administrative History of the NASA, 1958–1963* (Washington, 1966).

⁴⁰ Killian's *Sputniks, Scientists, and Eisenhower* is a memoir of great value. George Kistiakowsky's *A Scientist at the White House* is a diary graced by a long, excellent historical introduction by Charles S. Maier.

⁴¹ Killian, *Sputniks, Scientists, and Eisenhower*, 241.

primarily by scientists and became, thanks to administrator James Webb and the Apollo mission, a juggernaut of the engineers and politicians.

The answer seems to lie once again in the process of technological evolution itself. If artificial—that is, institutionalized—stimulation of science and technology has become a fundamental source of national power, then the political leadership needs to guide the allocation of technical resources according to its version of the national interest. Resistance to a political imprimatur over the creation of new knowledge had complicated legislation for the Atomic Energy Commission and National Science Foundation. But a command economy in state-funded research and development is implicit as political leaders move from choosing social goals for new technology to choosing new technology for social (and political) goals. This is what Eisenhower feared, for that meant abandoning the concept of a free society, which develops naturally according to myriad choices on the local level, and replacing it with a central directorate, which charts the path of social progress and orders fabrication of techniques for traversing it. This transition had already occurred in Western Europe (not to mention in the Soviet Union, which was founded on the principle), but it occurred in the United States only in the 1960s—and the catalyst was the space program.

Eisenhower shaped American space policy, but program funding can tilt the balance of policy. Kennedy defined the character of the American space effort with his commitment of May 25, 1961, to go to the moon. Thanks to Apollo, the space program came to stress engineering over science, competition over cooperation, civilian over military management, and prestige over practical applications. This crucial decision, the first turning point in the history of manned space flight, has been examined by John Logsdon.⁴² Why did we go to the moon? Why did \$30 billion go into Apollo technology instead of other space projects or nonspace applications? The answer lies not in technology, nor even in the Cold War itself, which might also have dictated a space program centered on military or economic applications, but rather in the conjunction of the early Soviet space triumphs with the emergence of the neutralist Third World. The prestige race to the moon—against the Russians if they were game, against the end of the decade if they were not—followed hard on reverses in Laos and the Congo, on the Bay of Pigs, and on the flight of Yuri Gagarin, first man in space. Vice-President Johnson condensed the wisdom of the new administration: “Failure to master space means being second best in every aspect, in the crucial area of our Cold War world. In the eyes of the world first in space means first, period; second in space is second in everything.”⁴³ So space technology was drafted, in the first months of the New Frontier, into the cause of national prestige.

Apollo was a magnificent achievement, but there is irony in the fact that observers worldwide consider it one of the “good” products of the maniacal 1960s.

⁴² Logsdon, *The Decision to Go to the Moon: Project Apollo and the National Interest* (Chicago, 1970). Also see Vernon Van Dyke, *Pride and Power: The Rationale of the Space Program* (Urbana, Ill., 1964). On the Apollo program itself, see the NASA histories: Courtney G. Brooks *et al.*, *Chariots for Apollo* (Washington, 1979); Roger E. Bilstein, *The Stages to Saturn* (Washington, 1980); and Charles D. Benson and William B. Flaherty, *Moonport: A History of Apollo Launch Facilities and Operations* (Washington, 1978).

⁴³ Johnson, as quoted in Hirsch and Trento, *The National Aeronautics and Space Administration*, 107.

"They Went Thataway"



Figure 3: "They Went Thataway," by Herblock. The Russian challenge and the necessary U.S. response are implicit in this cartoon, which appeared in the *Washington Post* on December 30, 1960. Reproduced courtesy of Herblock and Simon and Schuster from *Straight Herblock* (New York, 1964).

"Fill 'Er Up—I'm in a Race"



Figure 4: "Fill 'Er Up—I'm in a Race," by Herblock. This cartoon of May 24, 1961, preceded by only one day President John F. Kennedy's commitment to go to the moon by the end of the decade. Reproduced courtesy of the author and the *Washington Post*.

Contemporary critics on the Left and the Right considered such diversion of research and development to political goals of uncertain importance a misuse of the tool of state-induced technological change. By 1963–64 left-liberal critics denounced Apollo as wasteful given problems of racism and poverty,⁴⁴ while Barry Goldwater attacked the squandering of billions to impress Third World leaders. He bade America to remain secure in the intangible appeal of liberty and redirect the space program toward military and scientific applications.⁴⁵ In fact, the moon program was a synthesis of the same liberal mentality that conceived the Great Society at home and the same conservative one that supported containment of Soviet influence abroad. But in neither case did scientists play a significant role in policy.

Since 1957 governments have neither resolved the anomaly of democratic incompetence in technology nor abandoned the rituals of democracy. Instead, they continue to choose scientific advice just as voters choose the politicians, on the basis of promises, persuasiveness, personality, and their own preconceptions. Among the Edward Tellers, Wernher von Brauns, and Barry Commoners, whom do you trust? The Space Age introduced science and technology to the political arena, but it did not transform politics or usher the scientists to power.

FOR THE UNITED STATES, the early Space Age was a critical period of adjustment to the prospect of Soviet nuclear parity, to the emergence of a surly and disobedient Third World, and to the apparition of command technology as a genie in the service of government. Even if the Apollo program reflected the failure of science to reshape politics, it nonetheless unleashed hothouse technological change on an unprecedented scale. Apollo is the best symbol of a revolution in *governmental* expectations that occurred in the decade after 1957, and the roar of the rockets was its tocsin. The revolution in research and development dating from Sputnik can be termed, with only slight exaggeration, “Daedalus Unbound,” and here is the institutional and behavioral change that defines the Space Age in history.

The only analyst who has treated the early space program as an integral phenomenon rather than “just an expression” of this or that tendency in American life or the Cold War is Bruce Mazlish in his introduction to *The Railroad and the Space Program: An Exploration in Historical Analogy* (1965). Mazlish characterized the space program as a “complex social invention” that, like the railroads of the nineteenth century, was at once technological, economic, political, sociological, and

⁴⁴ See especially Amitai Etzioni, *The Moon Doggle* (Garden City, N.Y., 1964); and Edwin Diamond, *The Rise and Fall of the Space Age* (Garden City, N.Y., 1964). Also see Harold W. Babbitt, “Priorities, Frugality, and the Space Race: A Preliminary Assessment of Congressional Criticism of Project Apollo,” NASA HHN-41 (September 1964); William L. Crum, *Lunar Lunacy and Other Commentaries* (Philadelphia, 1965); Lester M. Hirsch, ed., *Man and Space* (New York, 1966); Erlend A. Kennan and Edmund H. Harvey, Jr., *Mission to the Moon: A Critical Examination of NASA and the Space Program* (New York, 1969); and John V. Moeser, *The Space Program and the Urban Problem* (Washington, 1969).

⁴⁵ Apollo did have indirect military benefits. Fear of technological surprise was a major impetus for McNamara’s and Rusk’s support of the moon program. It forged the “building blocks” of space mastery without the provocation of a large Air Force program.

intellectual.⁴⁶ If a major new technology or group of technologies catches on, its acculturation can involve pervasive social change. The space program, like the railroads, came to alter law, economic organization, and the fundamental relationship of the public and private sectors to make room for its growth. Like the railroad, the space program was also a cultural expression and influenced not only institutions but also intellectual and artistic sensibility.⁴⁷ In short, society was obliged to accommodate the new technological system, and the result was a social invention.⁴⁸

Can space programs be understood in this way, even though they apparently touch few people directly, involve a small percentage of gross national products, and seem absent from popular consciousness?⁴⁹ In fact, space technology is at the state of maturity that railroads had achieved in roughly 1860 or that radio had in the 1920s. Most Americans, pursuing their immediate goals in light of immediate experience, could not imagine the revolution aborning. Yet the patterns of social, economic, and political response to the spread of railroads, or radio, were shaped in their early decades. The railroads in particular have come to stand as the primary symbol of industrial take-off and the transition from agricultural to industrial society. How can we describe the postindustrial society associated with the Space Age? Zbigniew Brzezinski coined the term "technetronic" for a "society that is shaped culturally, psychologically, socially, and economically by the impact of technology and electronics, particularly computers and communications."⁵⁰ This neologism is problematical. Every society is shaped to some significant degree by its technology and method of communications, and few would argue that our society is shaped to a quantitatively greater degree, or in a qualitatively different way, than it was during the industrial early twentieth or agricultural eighteenth century. But technetronics do not aid us in understanding the origins or causal connections among the social phenomena of the new age. The familiar word "technocratic" will do instead.

What seems to have happened in the wake of Sputnik was the triumph of a technocratic mentality in the United States that extended not only to military spending, science, and space, but also to foreign aid, education, welfare, medical care, urban renewal, and more. Now, "technocracy" is a familiar word in modern history, meaning the management of society by technical experts. As such it is an ideal type, for such a society has never existed, not even in the Communist world. Despite the post-Sputnik boom in scientific advice, politicians and other influential groups manage society no matter how compelling the occasional technical con-

⁴⁶ Mazlish, ed., *The Railroad and the Space Program: An Exploration in Historical Analogy* (Cambridge, Mass., 1965), Introduction and 1–52, esp. 11–14.

⁴⁷ Leo Marx, "The Impact of the Railroad on the American Imagination, as a Possible Comparison for the Space Impact," in Mazlish, *The Railroad and the Space Program*, 202–16.

⁴⁸ Mazlish posited some generalities for historical inquiry: all social inventions are part of a complex in origin and result; none is uniquely determining in its impact; all aid some areas of development while blighting others; all develop in stages; all reflect a national "style." Mazlish, *The Railroad and the Space Program*, 34–35.

⁴⁹ American spending on space has fluctuated between 0.3 and 1.0 percent of gross national product; Soviet spending is estimated at a steady 1.5 to 2.0 percent; European spending less than 0.1 percent.

⁵⁰ Brzezinski, *Between Two Ages: America's Role in the Technetronic Age* (New York, 1970), 9–14.

straints. Let us, therefore, define technocracy as follows: the institutionalization of technological change for state purposes, involving the organization and funding by the state of a national infrastructure for the acceleration of technological change on the assumption that its own foreign and domestic goals will be served by the products of such change.

The transition in the United States to state-supported research and development for the continuous stimulation of technological revolution—this is the essence of the saltation that Sputnik triggered. The evolution of command technology is discernible in isolated cases dating from the eighteenth century. It became a regular feature of procurement in the British navy and German army in the 1860s, and it was vastly increased by all belligerents in the world wars.⁵¹ But Sputnik marked the discontinuity leading to full-fledged technocracy. In the United States, the Progressive era first provided a technocratic ideology, and World War II gave us the model of government-industry-science collaboration; postwar challenges to the passive role of government, the prestige of social science and Keynesian economics, and the American technique of softening social and ideological discord through policies expanding growth and opportunity all prepared the mix. Sputnik was the spark. By suggesting an imminent military stalemate between the superpowers and at the same time lending credibility to claims that Communism was a superior system for rapid national development, Sputnik changed the nature of the Cold War. It made it “total”—a global conflict in which science, education, housing, and medical care were as much measures of Cold War standing as human rights or bombers. Science was first: the creation of NASA, a deliberately civilian agency for purposes of propaganda, then the explosive growth of the National Science Foundation, and the National Defense Education Act. Apollo came next—it tripled NASA’s budget, channeled an extra \$30 billion into the aerospace industry and universities, and represented the largest open-ended peacetime commitment by Congress in history. The shock of Sputnik and Soviet claims to social superiority then helped to break down longstanding resistance by Republicans and Southern Democrats to federal involvement in education generally as well as other social arenas and began the greatest flood-tide of legislation in American history, a tide that, as Lyndon Johnson recalled, “all began with space.”⁵²

By the mid-1960s the space program helped convince investment-model economists and their colleagues in think tanks, in corporate boardrooms, and on Wall Street that heavy and systematic investment in technological and “human” resources was not a necessary evil but, rather, the key to continuous growth and, hence, social stability. Large social, military, and space expenditures would be covered by the new wealth created through new technology, thanks to the phenomenon of transfer—from point sectors (like aerospace and computers) to other sectors and from the American economy to developing economies. Forcing more rapid growth from an already superior scientific and industrial base, the

⁵¹ See McNeill, *The Pursuit of Power*, chap. 8.

⁵² The phrase “total Cold War” dates from a speech by Vice President Nixon in San Francisco, December 6, 1957. But especially see Eisenhower’s State of the Union Address, January 9, 1958. Johnson’s remark was made in an interview with Walter Cronkite, “Man on the Moon: The Epic Journey of Apollo 11,” July 21, 1969.



*Figure 5: "Earthrise," the famous shot of the earth beyond the rim of the moon, taken by the *Apollo 8* astronauts, Christmas, 1968. Photograph reproduced courtesy of the National Aeronautics and Space Administration, Washington, D.C.*

Technological Republic must outfight and outshine the Ideological Republics of Moscow and Peking.

The symbol and vanguard of the technocratic movement was NASA—efficient, daring, scientific, surely the locus of meritocracy. But the scale of the Apollo program called for more, for what James E. Webb dubbed a “managerial revolution.” Not only did NASA pioneer streamlined managerial techniques for integrating its own diverse projects, but it also formalized the links within the institutional triad of the federal agency, corporation, and university on which American society rests. This government-industry-university team, consciously cultivated by NASA, mobilized the nation’s human and material resources for “war” on the technological frontier.⁵³

For a time in the extravagant 1960s the American imagination exhibited that complacent exuberance attending the belief that one has magic, if not the gods, on one’s side. “The technological revolution that is now fully upon us,” wrote Webb, is the most decisive event of our times. . . . Unless a nation purposefully and systematically stimulates its technological advances into the sinews of the system, it will surely drop behind. . . . The great issue of this age is whether the United States can, within the framework of existing institutions, organize the development and use of advanced technologies more effectively than can the Soviet Union.

Success would stem from “our almost miraculous capacity to use existing technology to create new technology.”⁵⁴ Such encomiums were common. Even Adlai Stevenson proclaimed, “Science and technology are making the problems of today irrelevant in the long run, because our economy can grow to meet each new charge placed upon it. . . . This is the basic miracle of modern technology. . . . It is a magic wand that gives us what we desire!”⁵⁵

Miracles and magic—such faith helped to sustain a sixfold increase in federal obligations for research and development from 1955 to 1965. By the mid-1960s, the federal government had come to fund 80 percent of all research and development performed in the United States, and 90 percent of that under the aegis of the Department of Defense, NASA, and the Atomic Energy Commission. Organized change in the United States had been virtually nationalized. This saltation in the role of the state in creating new knowledge and power was itself transmitted abroad through the international imperative to accelerate technological, and thus economic and social, change elsewhere. Despite the vast lead and greater resources of the United States and Soviet Union, virtually every other major power on earth struggled to duplicate the rudiments of space technology and foster advanced aerospace industries. Public justifications of the vigorous space program of France or Europe as a whole, or of China or Japan, rested on the maxim that security and economic growth in the postmodern age of scientific-technological

⁵³ The science of managing large systems in fields promoting rapid innovation owes much to the experience of the Department of Defense and NASA in developing missile technology. General Bernard Schriever, chief of Air Force missile programs in the 1950s, and Kennedy’s appointments to head NASA and the Pentagon, James Webb and Robert McNamara, were instrumental.

⁵⁴ Webb, *Space Age Management: The Large-Scale Approach* (New York, 1969), chap. 2.

⁵⁵ Stevenson, “Science and Technology in the Political Arena,” in Xerox Corporation, *Science and Society: A Symposium* (New York, 1965).

revolution required that a state remain in the forefront or become hopelessly dependent and underdeveloped in the near future.⁵⁶ Gaullist France reacted to Sputnik by declaring the American nuclear deterrent unreliable and accelerating the drive for French nuclear and missile capacity. Soviet refusal to share missile technology aggravated the Sino-Soviet split and sparked a similar drive into space in China.⁵⁷ But the universal impulse to involvement in space was economic: the apparent technological gap that had opened by the mid-1960s precisely because it seemed the United States had discovered the “keys to power” in state-funded research and development in critical point sectors, which sustained technological revolution throughout the economy. *The Economist* wrote, “Prosperity depends on investment, investment on technology, and technology on science; ergo, prosperity depends on science.”⁵⁸ Charles de Gaulle exhorted France “to invest constantly, to push relentlessly our scientific and technological research in order to avoid sinking into a bitter mediocrity . . .”⁵⁹ French spending for research and development quadrupled during the first five years of the Fifth Republic, and France continued to lead Europe toward aerospace independence in order to overcome the technology gap, “brain drain,” and “industrial helotry.”⁶⁰

How does a society like that in the United States, or a smaller and less flexible one like that of France, absorb the effects of massive government expenditure for the ongoing creation of new technology? Most historians, whether they view new technology as a first cause or as a random joker in an otherwise ordered deck of historical cards, assume technology to be an independent stimulus of socioeconomic change, which in turn conditions (and usually disrupts) patterns of politics and diplomacy. This is arguable to some degree. But, if complex new technologies are sponsored by the state itself, then the state, whatever its ideology, becomes “revolutionary.” We would thus expect regimes to attempt to “socialize” new systems in such a way as to reinforce—not weaken—themselves, at least in the short run. This is the dilemma of the state obliged by foreign competition to foment technical change at home: not only to accommodate society to new technology but also to reconcile the two. Not surprisingly, de Gaulle promised to restore French

⁵⁶ On the European space agencies ELDO and ESRO in the 1960s, see Orio Giarini, *L'Europe et l'espace* (Lausanne, 1968); Jacques Tassin, *Vers l'Europe spatiale* (Paris, 1970); and Georges L. Thomson, *La Politique spatiale de l'Europe*, 2 vols. (Dijon, 1976). A model for study of social and economic effects of state stimulation of technological change is Robert Gilpin's *France in the Age of the Scientific State* (Princeton, 1968).

⁵⁷ Even under similar international pressures cultures historically responded to new technologies in different ways. See Eugene Ferguson, “Toward a Discipline of the History of Technology,” *Technology and Culture*, 15 (1974): 13–30. But in the 1980s, when China, Japan, and India are scrambling to replicate the nuclear and space capabilities of the Western powers, cultural differentiation in adoption and adaptation of new technologies may be eroding. Post-Mao China calls for only “Four Modernizations,” implying the intent to hold constant other features of Chinese civilization. How successful is this likely to be if the international imperative in the current technological age is becoming increasingly uniform in its impact?

⁵⁸ *The Economist*, October 5, 1963. Robert McNamara and others insisted that the real “gap” was in management, not in technology—that the systems approach, cost accounting, and computers sufficed to explain the advantage of the United States. See Roger Williams, *European Technology: The Politics of Collaboration* (London, 1973), 21–33.

⁵⁹ Charles de Gaulle, *Addresses to the French Nation* (1964).

⁶⁰ Jean-Jacques Servan-Schreiber best articulated French reaction to the technological gap in his *Le Défi américain* (Paris, 1968). Also see Pierre Vellas, *L'Europe face à la révolution technologique américaine* (Paris, 1969). Britain's confused reaction to the revolution in research and development is treated in Norman J. Vig, *Science and Technology in British Politics* (Oxford, 1968).

grandeur through explosive technological advance, but "without France ceasing to be France."⁶¹ And Webb promised miracles, but only through *existing institutions*.

Gaullist technocracy, therefore, was also a tool of domestic politics. By offering a captivating vision of the French future "in the year 2000," technocracy served to legitimate a Fifth Republic that had, after all, decreed the end of Imperial France and Socialist France and soon forestalled Atlanticist France and European France as well. Can a similar hypothesis be applied to the United States? This problem invites research: the emergence of a "revolutionary centrism" offering technological, not ideological or social, change to play midwife to a future as secure and bountiful, but less threatening, than that offered by either a socialist Left or a laissez faire Right.

The technocratic promise, of course, was not fulfilled, in part because its implicit dynamics were those of a perpetual motion machine. The "future" of the Great Society, like the final attainment of Communism in the Soviet bloc, never arrived. But material change has occurred on a massive scale. How can we measure the effects of Space Age technology? Econometrician Mary Holman analyzed the NASA budget and its impact on given localities (often very great), on economic growth and stabilization in the aerospace industry (positive), on the tendency toward centralization and monopoly in the industry (problematic), and as a stimulus to general economic growth (ineffective).⁶² Unfortunately, the best efforts of NASA itself ("more than any other federal agency NASA has tried to understand its social and economic impact"), the Organization for Economic Cooperation and Development, the National Science Foundation, and academic economists have been insufficient to measure the impact of spending on science and technology.⁶³ The economic cost of research in one sector depends on guesses as to the likely alternative employment of scientific labor and capital as determined by the state or marketplace. There is also no accepted set of values for spending directed toward no specific economic goal. What precisely is the state trying to achieve through federal scholarships, lunar voyages, or oceanography satellites? But one chore historians can undertake is to trace changes in the value-set of the "official mind" according to the educational, scientific, and technical projects that receive the blessing of public funding.

Estimates of the economic fallout from space spending still range from the minute to the cosmic. Critics described the space race as ceremonial waste,⁶⁴ and Holman has cited studies promising phenomenal benefit to cost ratios from earth resources satellites (for example, 128:1 in increased rice production and 296:1 in malaria control). And if we assume that space technology never existed (as Robert Fogel assumed away American railroad technology), the cost of alternate systems to perform given jobs is generally many times higher. Of course, mankind may get on

⁶¹ De Gaulle, Speech of February 5, 1962, as quoted in Gilpin, *France in the Age of the Scientific State*, 3.

⁶² Holman, *The Political Economy of the Space Program* (Palo Alto, 1974).

⁶³ *Ibid.*, 169–95. Raymond Bauer has suggested some procedures for measuring the social effects of space research, but his study was written too early for empirical analysis: Bauer, *Second Order Consequences: A Methodological Essay on the Impact of Technology* (Cambridge, Mass., 1969). Also see the excellent introduction by Wilbert E. Moore to Moore, ed., *Technology and Social Change* (Chicago, 1972), 3–25.

⁶⁴ A "potlatch" ceremony; Diamond, *Rise and Fall of the Space Age*, chap. 1.

decently enough without any system for surveying the resources of Amazonia or multiplying by millions the bits of information that can be transmitted between continents every few seconds. But it is another important historical conjuncture that the age of space technology arrived concurrent not only with the Cold War and the emergence of the Third World but also with the peak of the world demographic explosion, which seemed to demand accelerating economic growth from advanced nations to meet geometric increases in global needs. Still another inducement to experimentation with new technology is the rapid decrease in risk capital required after the preliminary breakthroughs—in this case, the plummeting cost per pound of placing hardware in orbit. The Saturn rockets of the mid-1960s had already enhanced cost efficiency a thousand times over the first boosters.⁶⁵ The Space Shuttle may reduce that figure even more significantly depending on the amortization schedule for the cost of development.

The most immediate impact of the space program was on the aerospace industry itself, which was declared “America’s newest giant” in 1962.⁶⁶ Individual NASA and Air Force program histories abound, but histories of the industry as a whole in all its political, economic, and labor facets in the United States and abroad, are nonexistent.⁶⁷ This is perplexing, for aviation had become by the 1930s an industry of vital interest to all the major powers, and it poses unique problems for historians of all stripes. Unlike most other industries, aerospace thrives on international discord. It requires vast excess capacity for emergency expansion, saddling firms with inordinate fixed costs. It has an unusually large percentage of highly skilled workers—at Boeing in the late 1950s over 40 percent of the employees were scientists and engineers. Aerospace is essentially a monopsony or oligopsony in which only one or two buyers exist (for example, NASA and the Department of Defense), and they provide both the market and the funds for research firms need to stay competitive. Hence, the industry must be an unabashed suitor of the state. Government agencies in turn have a stake in preserving competition among suppliers, but the very awarding of a large contract to one firm accords it a privileged position for the next assignment in the same field. Firms place a premium on grantsmanship not unlike the way professors learned to hustle in the regime of largesse after Sputnik. The effects of government patronage on universities in the United States and abroad are well known.⁶⁸ Similar effects in industry suggest the creation of a “contract state” in which private institutions rely

⁶⁵ Sir Bernard Lovell, *The Origins and International Economics of Space Exploration* (Edinburgh, 1973), 29–32.

⁶⁶ The Editors of *FORTUNE*, *The Space Industry: America’s Newest Giant* (Englewood Cliffs, N.J., 1962).

⁶⁷ Charles D. Bright’s *The Jet Makers: The Aerospace Industry from 1945 to 1972* (Lawrence, Kans., 1978) is a brief but incisive essay. Bright has also decried the lack of attention given one of the nation’s largest, most dynamic, and critical industries. A useful study from the peak of the Apollo boom is Herman O. Stekler, *The Structure and Performance of the Aerospace Industry* (Berkeley and Los Angeles, 1965). Stekler has denied that the American aerospace industry is a monopsony, citing the separate procurement processes of the armed services and the inordinate leverage accorded a handful of firms by the governmental practice of choosing prime contractors even for very large projects.

⁶⁸ In 1963 the federal cornucopia supplied 88 percent of the entire research budget of Caltech, 66 percent of MIT’s, 59 and 56 percent for the University of Chicago and Princeton, 24 percent for Harvard and Stanford; Etzioni, *Moon Doggle*, 68. The Denver Research Institute’s *Effects of a National Space Program on Universities* (Denver, 1968), a NASA-sponsored study, praised federal largesse, but found it to be more valuable to the universities than to the government!

on the government even as the state loses all hope of maintaining standards of cost and quality “given the revolutionary size, scope, and pace of the public interest in technological change.”⁶⁹

Once funding and contractual decisions fall by necessity to “experts” in arcane technical fields, once benign efforts at “technology assessment” are stymied by the very absence of shared values among scientists, engineers, businessmen, and bureaucrats, once the volume, scale, and complexity of projects invite cost overruns and unpredictable performance, then the state is demoted by its own magic to sorcerer’s apprentice. If this is not what Kennedy or de Gaulle or Khrushchev had in mind when they and their advisers seized upon technology as a political tool, then perhaps Mazlish’s expectation of stages through which a social invention must pass is borne out. Predictability of effects declines rapidly with the diffusion of new techniques and patterns of management throughout society. The special characteristics of aerospace and related industries suggest that traditional historical categories for policy, labor relations, investment patterns, and other phenomena are proportionally less applicable as integration or “interface” among state agencies and private or semiprivate corporations increases. Above all, it seems that government-industry-university teams to promote technology are inherently contradictory unless the conflicting values they embody are repressed.

The tendency of strategic, high-technology industries to alter the relationship of state and society is evident in Western Europe. Britain, France, Italy, and West Germany have all undergone almost complete concentration of their aerospace industries into semipublic behemoths under government pressure, so that the resulting giants might compete with each other and the large American firms. Monopsony has bred monopoly; exogenous pressures have shaped domestic institutions. Similarly, the Soviet Union, though socialist, actually fosters more competition since it can support several research centers that compete for party favor in design and production.⁷⁰

Take away the Cold War—and, hence, missile and space technology—and what would the American economy look like? This counterfactual question suggests that the journalistic debate on fall-out from the space program (NASA gave us the teflon pan, but was it worth it?) has hindered serious discussion of its historical impact. The role of space research as the intellectual, institutional, or financial progenitor of revolutionary developments in micro-miniaturization, computers, optics, materials processing, robotics, lasers, solar power cells, and more—this is the proper subject of the economic history of the Space Age.⁷¹ And the net gains from space technology should be measured not only against the total cost, or the economic cost, of the program itself but also against the continuing loss incurred

⁶⁹ H. L. Nieburg, “R & D and the Contract State: Throwing away the Yardstick,” *Bulletin of the Atomic Scientists* (March 1966), 20–24. Also see H. L. Nieburg, *In the Name of Science* (Chicago, 1966), chap. 10.

⁷⁰ See Parrot, “Politics and Technology in the Soviet Union,” chap. 5. Also see Alexander G. Korol, *Soviet R & D: Its Organization, Personnel, and Funds* (Cambridge, Mass., 1975).

⁷¹ Robert Fogel, in Mazlish, *The Railroad and the Space Program*, 106: “The consequences of space technology for the biological and physical sciences in general could lead to a technological and commercial revolution far more portentous than that which followed from the scientific breakthroughs of the seventeenth, eighteenth, and nineteenth centuries.”

from misdirected military and social spending encouraged by the same technocratic mentality that inspired Apollo. The advent of the technological fix, streamlined large systems-management techniques, compromise of the values embodied in once-autonomous social institutions, the dominance of government by political and social engineering—the entire drift of industrial democracies toward a materialistic, manipulative approach to public policy under the post-Sputnik infatuation with technique—these are also elements in the socialization of space technology.

Depending on the explanation of the comparative responses of regimes and societies to the challenges and temptations of the Space Age, some serious assignments present themselves: rethinking the relationship between capitalism and rationalization as it evolved in the new international technological environment after 1957; reconsideration (or rejection) of the notion of convergence between the political economies of East and West under the similar demands of expensive and complex technologies; and reformulation of the very equation of economic with political stability in an age of perpetual technological revolution.

THERE IS SUCH A THING as the Space Age—defined by the discontinuous leap in public stimulation and direction of research and development. Its ramifications have only just begun and they are already obliging us to set aside categories of political and economic history that have served more or less effectively for the whole industrial age. But have these phenomena and the existence and promise of ever more futuristic technology altered the bedrock of cultural values among nations? Is the advent of spaceflight capable, as Tsiolkovsky dreamed, of elevating mankind spiritually? Romantics after 1957 harbored such hopes. There was a certain symmetry in the notion that mankind's escape from the world itself must spawn a global self-consciousness, just as the Age of Discovery sharpened the self-consciousness and self-criticism (as well as hubris) of Europeans. But if space technology permitted some to visualize Spaceship Earth, it led others to see the enemy of cultural values in technology itself. Jacques Ellul argued that technology had so advanced that politics, economics, and art were not influenced by technique but rather were *situated in* a technical milieu, while a technological morality had long since supplanted inherited values.⁷² Space technology is an effervescence of the larger milieu that pre-exists and conditions its relation to modern culture.

Lewis Mumford judged space exploration to be "technological exhibitionism" and the latest expression of the "myth of the machine" that has dominated Western civilization since the twelfth century. Embalming astronauts in an artificial skin and blasting them into infinite vacuums in a skyscraper-tall rocket was for him the analogue to pyramid-building in ancient Egypt.⁷³ Sociologist Amitai Etzioni also interpreted space technology as the expression of an already flawed society:

⁷² Ellul, "The Technological Order," *Technology and Culture*, 3 (Fall, 1962): 394–421.

⁷³ Mumford, *The Myth of the Machine*, volume 2: *The Pentagon of Power* (New York, 1970), 303–11. Mumford's exquisite imagination failed him in this caricature of manned space flight. The "mummified astronaut" will be a primitive and romantic pioneer to the shirt-sleeved pilots and passengers of the next century, while the giant, throwaway chemical boosters of the early Space Age are already spurned as "big, dumb rockets."



Figure 6: "First Look," an oil painting by Mitchell Jamieson, commissioned by NASA. It is said to depict mankind's awe upon looking out to space; to me it seems, instead, to portray stark terror. Photograph reproduced courtesy of the National Aeronautics and Space Administration, Washington, D.C.

"Americans are apparently psychologically unready for peaceful coexistence and need to best the U.S.S.R. in everything," while the gigantic instruments that this adolescent insecurity demands only "serve those who seek to preserve the America of yesterday as it is confronted with the problems of tomorrow." For Norman Mailer the American space program belonged to "odorless WASPs," "the most Faustian, barbaric, draconian, progress-oriented, root-destroying people on earth." The machine had become art, the astronauts plastic men; and NASA's dubious accomplishment was "to make the moon boring." But Mailer equivocated. "For the first time in history a bureaucracy had committed itself to a surrealistic adventure." He vilified his own abominable army that debauched and dropped out while "they," with cool discipline, "have taken the moon."⁷⁴

These attacks could be matched with enthusiastic affirmations of the space effort and technological revolution from Buckminster Fuller, Krafft Ehricke, James Michener, and others. But whether positive or negative, such comments fall into two groups depending on whether their authors have interpreted the headlong flight of technology in our time as an outside force challenging and perhaps threatening historic culture or as the expression and fulfillment of culture, at least in the West. Is there a process of technological change that operates independently of value systems that would help to explain why Europeans came to explore the world and launch industrialism, or, indeed, why Chinese, Japanese, and Indians are now in such a hurry to get into space? Did Great Britain already have to be a "modern industrial culture" in some way for the factory system to spread, or did the spread of industry help change dominant British values?

We tend instinctively to assume that technological progress is a function of national values. Industrialism is somehow "Western" and the Apollo program very "American." But it is at least possible that our initial impulse is misleading. We also tend to assume that governments devise strategy by identifying their interests abroad and then marshalling the forces required to defend them. In fact, national interests are themselves a function of power—they observedly grow and shrink along with power potential, not vice versa. Similarly, the values of a given society may be in part a function of that society's power over its environment. Did the apparent power of command technology help to shape social values in the early Space Age? Or did the political decisions giving birth to the Space Age express something deeper and older than Sputnik, NASA, or the Cold War? Must the United States already have been "The Republic of Technology" of Daniel Boorstin's title, or the schizoid cult of hero and machine of John William Ward's intuition of the meaning of Lindbergh's flight, for the space technological revolution to have occurred?⁷⁵ Have our inherited values, material or transcendental, fed the geometric expansion of power? And if not, if our once-sovereign culture has become trapped within Ellul's technical milieu, then how did this metamorphosis come about?

In 1957, post-Sputnik American editorials drifted naturally between jeremiads

⁷⁴ Etzioni, *Moon Doggle*, Introduction, 152; and Mailer, *Fire on the Moon*, 10, 21, 31, 131, 346, 441.

⁷⁵ Boorstin, *The Republic of Technology* (New York, 1978); and Ward, "The Meaning of Lindbergh's Flight," *American Quarterly*, 10 (1958): 3–16.

and lamentations of lost technical supremacy. Ten years later the following was overheard at a State Department dinner: "All inventions for a long time will be made in the U.S. because we are moving so fast in technology, and large-scale, organized efforts produce inventions." The eavesdropper was James Webb; the speaker "a Mr. Brzezinski."⁷⁶ What had intervened to change a nation's mood was the space technological revolution. Our technological civilization has evolved for centuries. But the international rivalries of our age, culminating in Sputnik, induced a saltation in the politics of technology through the transformation of the state into an active, all-out promoter of technological progress. Alexander Gerschenkron theorized that the more economically backward a country, the more the state must play a role in forcing change. In the current age of perpetual and rapid progress, *all* states have become "backward" on a permanent basis. Hence, the institutionalization of wartime "emergency methods," the permanent suspension of "peacetime" values, the blurring of distinctions between the state and private institutions, and the apparent erosion of cultural differences around the world. History *is* speeding up, and the leading nations justify their ever-accelerating pace of innovation by the need to maintain military and economic security. Yet that very progress may, at times, undermine the values that make a society worth defending in the first place. This, succinctly stated, is the dilemma of the Space Age.

One is tempted to conclude that the creation and use of still more power as a solution to human problems is as vain as the effort of the American tourist to make his English understood by steadily raising his voice. "The worship of technology," wrote William C. Davidon after Sputnik, "has reduced the differences between totalitarian countries and those where human worth and dignity might be expected to find more devoted champions."⁷⁷ The fallacy of the early Space Age was that the pursuit of power, especially through science and technology, could absolve modern man from his duty to examine, affirm, or alter his own values and behavior in the first place. The politicians climbed aboard. It was left to Wernher von Braun to admonish "that man raise his ethical standards or perish."⁷⁸

⁷⁶ Memo, Webb to Frutkin, June 22, 1967, NASA Historical Archives.

⁷⁷ Davidon, "Soviet Satellites—U.S. Reactions," *Bulletin of the Atomic Scientists* (December 1957), 357–58. The contradictions of our "humanist religion" have been brilliantly exposed by David Ehrenfeld; see his *The Arrogance of Humanism* (New York, 1978).

⁷⁸ Wernher von Braun, a founder of the first Lutheran church in Huntsville, Alabama, and a disciple of Teilhard de Chardin, wrote the chapter on "Responsible Scientific Investigation and Application" in H. Ober Hess, ed., *The Nature of a Humane Society* (Philadelphia, 1976). It amounted to a political and moral testament since he died shortly after submitting it. Von Braun called for a new system of values transcending the "old American standards of material or technological efficiency."

Reviews of Books

GENERAL

HERBERT BUTTERFIELD. *The Origins of History*. Edited by ADAM WATSON. New York: Basic Books. 1981. Pp. 252. \$20.95.

At the time of his death in 1979, Herbert Butterfield had nearly completed the manuscript of *The Origins of History*. Adam Watson secured permission to edit and publish the manuscript, for which the profession can be grateful. Although Butterfield did not have time to expand some of the topics as he had planned, and even though he was unable to place his finishing touches on the manuscript, he nevertheless succeeded in producing a splendid work that contributes significantly to our understanding of the origins of historical writing.

In this work Butterfield traces the rise of historical consciousness in the West to ancient Mesopotamia, although he carefully shows that a sense of genuine historical awareness developed in ancient China as well. He argues that the development of "king-lists" and epics eventually led the Mesopotamians to construct interpretations of how things had come to be in a civilization faced with "discontinuity" in its past. In contrast, the ancient Egyptians failed to develop a sense of critical historical inquiry, primarily because of religious, political, and environmental circumstances. The Hittites, however, made significant progress in developing historiography, and, as Butterfield notes, their sense of history was probably unrivaled by any other ancient Western peoples except the Hebrews. Butterfield contends that historical consciousness first came to full flower in the land of Israel. This is not a new argument, of course, but Butterfield carries it to a higher plane. His critical, yet sympathetic, analysis provides a clear picture of the Hebrew conception of linear progression. In contrast, Butterfield claims, the ancient Greeks, despite their remarkable achievements in philosophy and critical inquiry, generally cut themselves off from their past and thus failed to make any significant impact on historiographical developments.

The author traces the evolution of Western historiography through the Christian era, the Middle Ages, and the Renaissance; he explores the "secularization" of historical writing; and he touches upon the rise of critical history in the eighteenth and early nineteenth centuries. He also includes short though insightful accounts of the origins of Chinese and Islamic historiography, but the latter is so brief that it does little to enlarge our understanding.

Although Butterfield read widely on the subject of the origins and evolution of historical writing, he either overlooked or did not have time to consult other useful works. Among those that could possibly shed light upon the subject is Joseph Campbell's *The Masks of God: Occidental Mythology*, which demonstrates the influence of various sects upon Christianity. Many of those sects brought ideas from the land of Mesopotamia and beyond, but we are left to wonder to what extent their views may have helped to shape Christian historiography since Butterfield does not mention them. Nor does he appear to be familiar with Page Smith's *The Historian and History*, which offers some of the same arguments he puts forward, even if in much briefer form. Moreover, Butterfield does not cite Donald R. Kelley's *Foundations of Modern Historical Scholarship*, which thoroughly examines the development of critical historical inquiry in the French Renaissance. Nevertheless, Butterfield has written a survey that does more than any other single work to explore the beginnings and the development of historiography. *The Origins of History* is an excellent contribution to the field.

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EUGENE WEBB. *Eric Voegelin: Philosopher of History*. Seattle: University of Washington Press. 1981. Pp. ix, 320. \$20.00.

Eric Voegelin's radically different stance has kept his philosophy of history from being appreciated by the mainly neopositivist and analytical philosophers of history in the United States and England. There

have also been other obstacles to Voegelin's approach, such as its complex philosophical matrix, its idiosyncratic vocabulary, and its scattered presentation in many articles, sections of *The New Science of Politics* (1952), and the four-volume work, *Order and History* (1956–74). Perhaps Eugene Webb's excellent book, which offers a lucid exposition of Voegelin's thought and includes a glossary of Voegelin's terms, can remove some of the obstacles that make understanding Voegelin's philosophy of history so difficult.

Proceeding from the Augustinian tenet that the structure of history is identical with the structure of personal existence, Voegelin explored at great length the human condition and found at its core the "tension of existence." This is caused by two opposing forces: the pull toward transcendence, experienced as the longing for perfection familiar to Platonic philosophers and Christians, and the pull toward complete immanence that tries to quiet that longing under various pretenses. In the proper life, the "open existence," the individual affirms transcendence by avoiding all "closures" (systems) and by rejecting the illusion of "final" knowledge.

Voegelin's attempt to construct his philosophy of history in a chain of reasoning that begins with his basic views on the human condition has induced Webb to analyze the whole intellectual matrix of Voegelin's philosophy in order to demonstrate how his philosophy of history formed an integral part of Voegelin's world-view. Such an approach leads Webb to present Voegelin's historical thought in a manner that stresses its systematic coherence and de-emphasizes its gradual formation and internal tensions. These issues have been raised more consistently and precisely in reviews of Voegelin's books and in Stephen A. McKnight's *Eric Voegelin's Search for Order in History* (1978).

Voegelin's crucial problem in formulating his philosophy of history was to avoid presenting world history either as the mere aggregate of generations of individuals striving toward transcendence—a directionless continuum—or as an image of immanentist, speculative systems à la Marx and Spengler, with their inevitable claim to total knowledge, diminution of the richness of human experience, and illusion of final "stability." In his answer Voegelin considered history to be the story of how human existence—as the truth always fully present—has been variously experienced and expressed in symbols. History is "a trail of equivalent symbols." Yet this modern version of Herder and Ranke's equidistance to God does not forestall history as the anarchy of discrete views that so troubled the aging Dilthey and subsequent historicists. Thus Voegelin added direction through the sequence of three types of symbolisms (cosmological, anthropological, and soteriological) that offer an increasingly fuller

comprehension of the truth. In a slightly different vein he has spoken of the development of consciousness in the direction of greater differentiation. But all along he has still reaffirmed equivalence and so left unresolved a key problem in his philosophy of history.

ERNST BREISACH
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DALE H. PORTER. *The Emergence of the Past: A Theory of Historical Explanation*. Chicago: University of Chicago Press. 1981. Pp. x, 205.

According to Dale H. Porter, the way events emerge in the present is neither completely determined nor undetermined. It follows, Porter argues, that history cannot be explained mechanistically in terms of causes and effects. Consequently, he sees the narrative as the form most suited to the historian's subject matter. But though it may be impossible to explain history according to a strict causal pattern, Porter admits that the stories that replace the quasiscientific presentation of the material need to be accounted for and their validity assessed. As a basis for such assessments, Porter does not take the beaten path of discussing the nature of the arts and the (social) sciences but inquires into the nature of historical events.

For many historians Newtonian time has formed an unquestioned, if also largely unconscious, point of departure. This conception turns all events into discrete entities, whose succession is ruled by mechanistic principles. Similarly, historians have described events as discrete entities located in time, the succession of which could be explained "scientifically" by an appeal to historical laws. Dissident historians have intuitively resisted this kind of reductionism. But they have failed in defending their discipline against the accusation of "immaturity." Thus history remained in the eyes of the scientifically minded an art, and that meant to them a regrettable subjective enterprise. According to Porter, this whole debate, however, has been misguided from the start, and now that Einstein's conception of time has replaced that of Newton this should become clear to all. Contemporary scientists see time in terms of relations, or as part and parcel of a given entity. This conception of time fits what idealist historians have been saying all along: that mechanistic explanations are out of place in history. In order to reorient the debate, Porter presents a "process approach" to historical explanation. He introduces thereby the theories of Whitehead into the philosophy of history.

Porter considers events not as static entities having once and for all a definite form and significance, but as changing temporal structures. Their changes

depend on the perspective of succeeding events—of which the latest one is the historian writing history. Just as the organic structures of biology, as a result of their capacity to respond to signals, seem to behave intelligently and as if they were endowed with consciousness, so the temporal structures of past events such as the French Revolution or the passing of the Emancipation Act (on which the author published a narrative) change their form and significance, responding as it were to signals from later times. Events, in other words, are not what they are but what they become, and this not only in the eye of the beholder but in actual fact. In this process model, Porter claims, determinism and indeterminism and necessity and freedom are accounted for, and this clarifies why the telling of stories is so congenial to historians.

In order to facilitate the required accountability of the historian's narratives, Porter studies the kinds of events that historians encounter in history and classifies them according to their structure and behavior. He then outlines fourteen plot forms, seven levels of abstraction and ninety-eight categories. To the objection that this is excessively generous, Porter answers that it is "modest compared to the 25,000 categories of spiders alone in biology" (p. 159).

Unfortunately, Porter's presentation of his reasoning is somewhat chaotic. The book's most substantial part, the reflections on the nature of historical events, do not form its center, and the services of another philosopher of history will probably be needed in order to explain their importance to the working historian. Again, Porter makes no reference to other researchers in the field other than analytical philosophers of history. He could have strengthened his position by showing how his views are related to those of Hayden White, contemporary philosophers and historians of science, and also perhaps Continental hermeneutic philosophers. As it is, there is a danger that the book will be overlooked by historians who otherwise might have considered to use his suggestions as points of departure for their own work.

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JEFFREY BURTON RUSSELL. *Satan: The Early Christian Tradition*. Ithaca, N.Y.: Cornell University Press. 1981. Pp. 258. \$19.95.

This is the second of two volumes in which Jeffrey Burton Russell has treated the history of the idea (or, as Russell would prefer, the "concept"; see pp. 19–29) of the devil as the personification of evil. The first volume (*The Devil: Perceptions of Evil from Antiquity to Primitive Christianity* [1977]) began with a

wide-ranging inquiry into the comparative cultural concepts of evil and narrowed in the last three chapters to a specific study of the figure of Satan-Devil in Hebrew and New Testament sources. After a first chapter that revises somewhat Russell's earlier theory of concepts, this book begins with the Apostolic Fathers and proceeds in six chapters through the work of St. Augustine, "by which time the main lines of the tradition had been established." Russell makes two claims for this volume: first, that it should help "gain a better understanding of the nature of evil"; second, that it "develop[s] a method for the history of concepts and the historical theology of concepts." The first claim is unreviewable in this professional journal, but it must be said that Russell does help interested historians to gain a better understanding of how the early church perceived and defined evil and the devil.

The second claim is properly historical-methodological, although the history of theology seems somewhat less tractable to the kinds of revisionism in the history of thought that have been so effective in the works of Kuhn on the history of science and Pocock on political discourse. Russell attempts to sort out a confounding of intellectual functions that has often plagued the history of thought and may be seen clearly in the most reductionist work of Lovejoy and the most recent work of Nisbet. Russell avoids much of this because he is aware of the dangers of both reductionism and the power of "future focus" on the historian's work. His definition of a concept is more limited here than in his first volume. Mutual influence draws individual human "notions of an entity" (p. 20) into a "constellation." Surviving constellations form a "tradition." "The historical tradition of human perceptions of an entity . . . constitutes a concept." Whatever methodological and conceptual arguments one may raise to question this definition, one must agree that Russell firmly identifies concepts as human artifacts that may diminish, change, or grow over time, although his insistence on the independent existence of an "entity" and on the whole tradition as constituting a "concept" will not meet with universal agreement.

Arguments about definitions do not, however, detract from the value of this book. Russell has a far more manageable period to work with here, one in which an intellectual tradition (in which the concept of the devil was simply one part) took shape and exerted influence for many centuries. Russell's chapters on the Apostolic Fathers, the apologists and the gnostics, Irenaeus and Tertullian, the Alexandrian theologians, the theology of the desert fathers as perceived by Evagrius and Athanasius, and St. Augustine all cover ground universally recognized by historians and theologians as the seed-bed of Greek and Latin Christian theology. A useful aspect of Russell's wider concern with the general

problem of evil seems to have led him to aim at a much wider audience than patrologists alone, and his identifications of individual thinkers and works is extremely helpful to the nonspecialist; citations in Greek and Latin should not deter the reader. Russell also knows how many early Christian "constellations" that do not become orthodoxy nevertheless exerted considerable influence on later medieval literature and art, and he is generous in pointing out many of these. Russell's definitions of "tradition" and "concept" are sufficiently broad to include the history of liturgy and moral theology. Although art before the sixth century offers him no examples, his treatment of later art is well within the limits of acceptability, particularly since nonverbal representations played a considerable role in European culture from late antiquity through the Middle Ages. Drawing extensively on earlier scholarly literature as well as his own original research in complex source materials, Russell has offered a coherent account of the development of a tradition in Christian thought that should be of great interest to specialists and nonspecialists alike. Although Russell would be the very last to claim that he can draw out leviathan with a hook, he has competently and diligently drawn out an image of leviathan that takes a respectable place in the literature of early church history.

EDWARD PETERS
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EDWARD GRANT. *Much Ado about Nothing: Theories of Space and Vacuum from the Middle Ages to the Scientific Revolution*. New York: Cambridge University Press. 1981. Pp. xiii, 456. \$59.50.

For the past twenty years, Edward Grant has been investigating concepts of space and vacuum in Western thought. This book is the fruition of his research. Beginning with the views of Aristotle and those philosophers he discussed, Grant then jumps to the mid-thirteenth century, and from that point on offers us a full, clear, and coherent account of European concepts of space between the thirteenth and eighteenth centuries. This is a rich and complex book, doing justice to the intricacies of its subject and making clear by implication the basic role played by concepts of space in the scientific and philosophical thought of Europe. After an analysis of the Aristotelian position, it investigates medieval notions of vacuum, space, place, and motion, then traces the concept of an infinite extracosmic void from Archytas of Tarentum through the scholastic writers of the seventeenth century, and concludes with an extremely valuable and thoughtful chapter on the nonscholastic thought of the sixteenth to

eighteenth centuries. There are quite a few surprises. Euclid, Nicholas of Cusa, Copernicus, and Galileo are shown to have had little or nothing to do with the development of concepts of space. Bradwardine, Jean de Ripa, Oresme, John More, and a host of virtually ignored late scholastic authors were extremely important. Pierre Gassendi (1592–1655) appears as the crucial figure in the development of spatial doctrine because "he merged ideas from scholastic and nonscholastic sources and exercised an influence directly, or indirectly through Walter Charleton, on Pascal, Barrow, Newton, and Locke. Not only was Gassendi's cosmology a hybrid drawn from Greek Atomism and Stoicism, but his concepts of space united scholastic concerns about God and infinite imaginary space with the by-then standard three-dimensional infinite space of nonscholastics" (p. 207). Also, the relationships to the earlier traditions of such heroes of modern thought as Descartes, Newton, and Spinoza are shown to be quite other than one might expect.

Excellent though it is, the book would have been improved if more attention had been paid to the demonstrable intellectual affiliations among the authors discussed (Grant does this sporadically but not consistently), and if the prescholastic period of medieval thought, particularly the twelfth century, had been taken more seriously. There are a few slips: Archytas of Tarentum lived in the fourth century B.C., not A.D. (p. 106); Lucretius was not "[p]reviously known only through the biased reports of Aristotle" (p. 183); and English usage still requires that a gerund be modified by a possessive.

This is a work of major importance, more likely to stimulate further discussion of its subject than to quell it. It has built a bridge across the chasm separating medieval from modern historians. And it is a model of careful, thorough analysis of texts in tracing the vagaries of Western examinations of the meaning of space, a concept that is central to much of Western thought.

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DERRICK BAXBY. *Jenner's Smallpox Vaccine: The Riddle of Vaccinia Virus and Its Origin*. London: Heinemann Educational Books. 1981. Pp. xiv, 214. \$25.00.

Derrick Baxby's book is the third one to appear within the past four years that attempts to explain the origin of the modern strain of vaccinia virus. The first two books, *Edward Jenner's Cowpox Vaccine: The History of a Medical Myth* and *The Conquest of Smallpox*, appeared in 1977 and were written by Peter Razzell. In these two works Razzell tries to deny Edward Jenner credit for introducing vaccina-

tion. Around 1720 the practice of inoculation was introduced into the Western world. This consisted of inoculating healthy individuals with pus from an active smallpox case. Those undergoing this procedure usually had a mild form of the disease and were subsequently immune to it. Razzell maintains that the extensive use of smallpox inoculation led to an attenuated form of smallpox and that this development coincided with Jenner's supposed introduction of cowpox or vaccine. This is not the first time that efforts have been made to discredit Jenner. For example, Charles Creighton, possibly the leading British epidemiologist in the late nineteenth century, denounced Jenner in 1889 as a charlatan.

Baxby, the author of *Jenner's Smallpox Vaccine*, is a senior lecturer in medical microbiology at the University of Liverpool who has spent almost twenty years studying the various poxviruses related to smallpox. Smallpox is a member of the genus *Orthopoxvirus*, which includes cowpox, vaccinia, monkeypox, camelpox, and buffalopox. Their one common characteristic is that infection with any one confers immunity to the others.

Baxby's own work and that of other virologists have confirmed that the vaccinia virus is distinct from both cowpox and smallpox. The problem, then, is to account for vaccinia. Several theories have been put forth to explain its origin, but the essential arguments are that it is an attenuated form of cowpox or smallpox or that it represents a genetic hybrid derived from smallpox and cowpox. Recognizing that vaccinia is distinct from cowpox and yet convinced that Jenner was responsible for introducing a relatively safe preventive for smallpox, Baxby retraces the early history of vaccination, attempting to follow the successive strains of vaccine material. Although his evidence is somewhat limited, he suggests that Jenner actually used horsepox, a disease referred to on several occasions by Jenner, but one that was eliminated by the end of the nineteenth century.

It is difficult for medical historians, even in the case of epidemic diseases, to make positive diagnoses, and without access to Jenner's vaccine matter the origin of the present strain of vaccinia can never be known. Baxby provides an excellent history of vaccination, and his theory has a measure of plausibility. The book is well written and presents a clear overall view of the introduction of vaccination, the subsequent historical debates over Jenner's role, and the current knowledge of the poxviruses.

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SEYMOUR H. MAUSKOPF and MICHAEL R. MCVAUGH.
The Elusive Science: Origins of Experimental Psychical

Research. Afterword by J. B. RHINE and L. E. RHINE. Baltimore: Johns Hopkins University Press. 1980. Pp. xvi, 368. \$24.50.

Seymour H. Mauskopf and Michael R. McVaugh seem to have made a bet that parapsychology, or "psi," will become an important part of mainstream science. If this someday occurs, their volume may be looked back on as a landmark contribution to the documentation of psi's early experimental decades. But if the authors lose this bet, their book will likely be regarded as a technically competent tracing of a scientific sideshow. Even if the bet is won their treatment is unfortunate, however, because the authors obdurately refuse to theorize about their material. They present a highly detailed and apparently quite accurate chronology, but with almost no interpretation. More's the pity, since parapsychological research provides a compelling occasion for understanding the dynamics of scientific deviance and legitimization. In the space remaining for this review, I will summarize the contents of *The Elusive Science* and then sketch the theory of "science deviance" that is suggested by this data.

As its title implies, *The Elusive Science* discusses the transformation of interest in psychical phenomena from an amateurish and "spiritualist" enterprise to one that is thoroughly scientific, even scientific, in its methods. The authors concentrate on the decades between 1920 and 1940 when psychical researchers, led principally by the late J. B. Rhine, made a sustained effort both to scientize their investigations and also, in so doing, to gain acceptance by mainstream academic psychologists. This story is told in great, perhaps excessive, detail, using source material from previously unexplored archival collections of the British and American societies for psychical research, as well as from the collection of Rhine's papers and correspondence at the Duke University parapsychological laboratory, the leading center of parapsychological research in the United States.

Salient features of this story are never explicitly highlighted by the authors, but they can be drawn from their narrative to form an explanation of the chronological events that are recounted. A principal such feature is the definition of psychical phenomena given by parapsychological researchers themselves—psi phenomena are those that cannot be explained by the laws of conventional physical science. In so defining their subject matter, however, psi researchers have in effect defined themselves out of normal science. Given this, the interesting question for a broader history of science becomes exactly the opposite of that posed by Mauskopf and McVaugh. This question is not why psi research has not become accepted, for by definition it is just the

sort of anomaly that normal science dismisses or overlooks. Instead, the perplexing fact is that parapsychology has stayed around as long as it has. Unlike N-rays and the Baxter effect, which were dismissed as frauds or errors, and unlike "falling stones" and mesmerism, which were absorbed into mainstream science as meteorites and hypnotism, parapsychology has retained its status as a "deviant science" for almost a century. Why is this so?

Recent research by James McClenon (publication forthcoming) suggests two major reasons, both stemming from the fact that parapsychologists have rejected the physicalistic ontology that continues to dominate the world-view of orthodox scientists. First, even though eschewing physicalistic *explanations* of the phenomena that they study, parapsychological researchers employ a hyperphysicalistic *methodology* in their investigations. In effect, they request admission into orthodox science with their left hand while rejecting it with their right. The second reason for the continuing durability of psi's marginal status is its popular support. As science has become a dominant rhetoric in the guidance of more and more areas of social praxis—scientific management, scientific warfare, scientific welfare—there has grown a corresponding unofficial resistance to such domination, along with a resentment of the arrogance of scientists that is thought to accompany it. Hence, there is a steady stream of interest in and financial support for parapsychological research from the general public, support sufficient to keep parapsychologists in business. The irony, of course, is that this very support inhibits the acceptance of parapsychologists by mainstream scientists, because the openly "spiritualist" leanings of lay philanthropists taints any research that they fund as antiscientific.

Parapsychologists thus are caught in a conundrum. If they were to abandon their antiphysicalistic definition of their subject matter they could lose their *raison d'être* as a separate scientific entity and a recipient of lay contributions. Conversely, if they were to abandon their scientific methodology they would gain more popular support but lose all chance of professional acceptance. Thus, parapsychologists find themselves twice stalemated. On the one hand, having been labeled as deviant science, their work serves to mark the boundaries of what is considered legitimate as scientific inquiry, thereby fulfilling a function of interest to orthodox scientists. On the other hand, this work also serves as a protest against mainstream science in the eyes of lay philanthropists. What an Iranian Bahá'ist said of his Islamic countrymen is perhaps true of parapsychologists' relationship to both orthodox scientists and lay funders: "They keep us alive in order to kill us."

The acceptance of psi by mainstream scientists seems to be the *telos* of Mauskopf and McVaugh's

book. But this *telos* is unlikely to be fulfilled unless either of two conditions are met. First, there would have to be a dramatic breakthrough in parapsychological research itself, either one that assured consistent and predictable rates of reproduction of psi phenomena even by unsympathetic researchers or one that provided a convincing theoretical explanation of such results as have been produced. Both such breakthroughs seem unlikely, for the very scientism of parapsychologists' methods tends to lower rates of psi, narrow the range of phenomena studied, and direct attention away from theoretical inquiry. The second condition that might lead to full legitimization would be a basic shift in the criteria of legitimization—that is, a shift in the larger definition of what science and reality are themselves. This of course inheres in the broader culture and, as such, is not subject to direction by parapsychologists.

It thus seems likely that legitimization of parapsychology will remain elusive and that the history of this inquiry will be one neither of progress in the direction of clear acceptance nor of definitive stigmatization. Instead, we can expect continuing alternation between near acceptance and repeated rejection, an alternation imposed by the social, structural, and ideational forces at work.

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KNUD HAAKONSEN. *The Science of a Legislator: The Natural Jurisprudence of David Hume and Adam Smith*. New York: Cambridge University Press. 1981. Pp. viii, 240. \$34.50.

Students of the Scottish Enlightenment are indebted to Knud Haakonssen for this excellent monograph on the theories of justice of David Hume and Adam Smith. Haakonssen's study offers convincing evidence that Hume's concept of justice played a crucial role in shaping Smith's philosophy of jurisprudence, both as it appears in his theory of ethics and in the social philosophy of *The Wealth of Nations*. Haakonssen's treatment relies on a close and careful reading of the works of Hume and Smith that is, at times, extremely technical, yet his exposition is consistently penetrating and lucid.

With respect to his analysis of Hume's notion of justice, Haakonssen shows that the concept of justice as an artificial virtue—as Hume understands the term—is not inconsistent with some aspects of natural law theory. Although the rules that comprise justice for Hume are the product of convention, they are not capricious and are, in fact, grounded in man's nature as a social animal born into a universe of scarce resources. There are thus certain values that all men share by nature of the fact that they live in a common environment where the desires of all

men exceed the means to satisfy them. It is this condition that gives rise to the fundamental rules of justice—what Hume designates as the “fundamental laws of nature.” By so arguing, Hume is able to escape a relativistic legal theory, which he so strongly condemns throughout his works.

The most important aspect of Hume’s discussion in terms of its influence on Smith’s views concerns his analysis of the method by which these conventions come into being. The rules of justice are not the conscious and deliberate creation of all members of society at a specific moment in time but rather evolve as the result of countless individual actions, each aimed at distinct private ends; justice, then, is the result of human action but not of human design. Haakonssen describes this formulation as “one of the boldest moves in the history of the philosophy of law,” which avoids a legal positivist position while permitting Hume to embrace a species of natural law theory (p. 20). For, as Haakonssen points out, justice—having as its primary objective the protection of private property—is an artificial contrivance in Hume’s theory only in the sense that it requires the mediation of man’s reasoning faculty and not because it is not reducible to natural causes that give rise to natural laws.

Smith employs a similar approach in his account of acquired rights, among which he includes property, and in describing the origin of general rules of morality, which are themselves “the unintended outcome of a multitude of individual instances of natural moral evaluation” (p. 61). The theory of moral obligation and duty and the theory of property, from which flow their respective theories of justice, are thus markedly similar in both Hume and Smith.

The major portion of Haakonssen’s book is devoted to a detailed examination of Smith’s analytical, critical, and historical jurisprudence, including an extensive discussion of Smith’s concept of rights, both natural and adventitious, and his theory of punishment. In his treatment of Smith’s jurisprudence within the context of his theory of historical development, Haakonssen rightly takes strong exception to the widely held view that Smith subscribed to a materialist conception of history. That societies pass through certain stages of development, each marked by distinct economic institutions, does not, Haakonssen insists, entail the notion that economic factors are either the sole or crucial determinants in giving shape to other social arrangements or in motivating human action. These attempts to make Smith (and other Scottish Enlightenment thinkers such as Adam Ferguson) proto-Marxist are doomed to failure once his theory of historical development is analyzed against the backdrop of his ethics and jurisprudence.

Haakonssen’s study is a welcome contribution to

the history of Scottish Enlightenment thought and deserves careful reading by all historians and philosophers interested in the period.

RONALD HAMOWY
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JOHN P. DAWSON. *Gifts and Promises: Continental and American Law Compared*. (Storrs Lectures on Jurisprudence.) New Haven: Yale University Press. 1980. Pp. ix, 240. \$16.95.

John P. Dawson’s *Gifts and Promises* is a skillful and complex study of a highly specialized topic: development and comparison of Continental (French and German) law and Anglo-American law governing gifts and promises. Originally the 1978 Storrs Lectures at Yale Law School, the book consists essentially of four tightly woven essays, one each on Roman, French, and German law relating to gifts, and one on the Anglo-American doctrine of consideration, seemingly a concept the very opposite of gift.

The Justinian Code provided that if the promisor of a gift attempted to retract, that promise could be enforced. The church brought strong pressure to expand the range of enforceable gifts, since breach of promise was a sin. The big exception were gifts between husband and wife, which were absolutely void. Dawson suggests that the prohibition reflected concern over diversion of assets from one family line to another. Thus Roman law, while giving enforceable status to promises of gifts, at the same time narrowed the range of possible gift giving.

In France this dual development of enforcement and restriction included by the seventeenth century the Roman institution of the *legitime*, by which certain classes of heirs were entitled to a percentage share of an estate, irrespective of disposal by will and irrespective of *inter vivos* gifts. Public recording or notarization was required for all gifts. These practices survived the Revolution and became embedded in the Civil Code.

Dawson’s chapter on Germany underlines the divergences with France. Both countries accepted the contractual enforceability of gifts, but the German system required notarization only for promises and limited the postaudit of a decedent’s gifts to ten years before death. Dawson is decidedly more favorable to the German model, which avoided the rigidity (and compensatory deviousness) of the French practices.

His chapter on the Anglo-American doctrine of consideration is loosely connected to the main themes of the book. Dawson asserts that we have little to learn from the Continental experience because we developed early the element of “bargained-for exchange” to serve as adjunct to an economic system where private expectations became

the central element of contract. Once contract became associated with consideration, it was inevitable that promises without consideration would have no legal status, with a few exceptions.

Dawson's book is especially valuable for scholars in comparative legal history, where it supplements his highly regarded *The Oracles of the Law*, which compares judicial decision making in four legal systems. This reviewer missed a chapter that would set the topic in the broader historical and jurisprudential framework of the evolution of the civil law. Footnotes are helpfully at the bottom of the pages, and there is an interesting appendix showing the distribution in Europe and Latin America of the system of guaranteed shares in inheritance, which Dawson considers the central theme of Continental controls over gifts.

ARNOLD MILTON PAUL
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PAOLO GROSSI. *An Alternative to Private Property: Collective Property in the Juridical Consciousness of the Nineteenth Century*. Translated by LYDIA G. COCHRANE. Chicago: University of Chicago Press. 1981. Pp. xv, 344. \$34.00.

One of the signal achievements of intellectual historians and historically minded social scientists in the nineteenth century was the discovery of what the publicist Carlo Cattaneo once called "*un altro modo di possedere*." Delving into the ancient laws of the Germans, the Slavs, the Hindus, and the Celts, scholars found evidence that impugned the faith of the Enlightenment in the individual as the only holder of property by natural right. They described modes of collective ownership no less ancient, and perhaps more so, than the "possessive individualism" of classical economics. This discovery of alternative concepts of property and its practical application in Italian legislative reform in the late nineteenth century is the theme of Paolo Grossi's study, first published in 1977 and now made available in a handsome translation by Lydia G. Cochrane.

Grossi is professor of the history of Italian law at the University of Florence and the director of its Institute of the Theory and History of Law. His work is not well known in the English-speaking world, a state of affairs that the timely publication of *An Alternative to Private Property* should do much to remedy. He divides the volume into two parts of equal length: the first, an impassioned review of the relevant scholarship, with emphasis on the English jurist Henry Sumner Maine and the Belgian economist Emile de Laveleye, and their implacable French critic N. D. Fustel de Coulanges; and the second, a discussion of how the debate in the northern coun-

tries was reflected in Italian legal thought after unification, and how it played an important part in the law of 1894 that recognized the property rights of communal associations in the former papal states of central Italy. Grossi writes intellectual history as it should be written, as ideas emerging from a broad context of thought and action, having their effect in specific historical situations.

The only difficulty with the book is its inconsistency on the role of ideology in scholarly inquiry. On the one hand, Grossi condemns liberal (and, by implication, Marxist) explorations of the idea of property in the nineteenth century, in large measure because of their partisan subjectivity. No one has ever done a better job of exposing the human failings of a covert bourgeois apologist such as Fustel de Coulanges. On the other hand, Grossi himself adopts the pose of the objective scientist, loftily above the ideological *mélée*, when in fact he is an ardent supporter of the romantic-conservative camp in juristic thought. Every adjective in his abundant vocabulary warmly presses the case for Maine, Laveleye, and all their followers, just as it makes fools of their "sclerotic" and "rigid" detractors. The contrasts are so black and white that even the most innocent reader is tempted to draw back and ask for a second opinion.

But this is a valuable work, within its limits. Although partisan, it is never narrow or fussy. Its theme should be of considerable interest to students of nineteenth-century historiography as well as of legal and economic thought. It is further enhanced by Julius Kirshner's sympathetic foreword and by an unusually generous supply of bibliographical notes.

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FRANCESCO CARACCIOLO. *Il processo di industrializzazione: Politica economica e riflessioni teoriche sulla crescita industriale nei paesi second comers*. Rome: Gentile. 1979. Pp. 510. L. 16,000.

This is a highly literate collection of essays on economic relations between the developed (England) and developing (Continental) countries between the late eighteenth and mid-nineteenth centuries. It contains chapters on industrialization in England, the U.S., France, Italy, and Germany; the debate about protection and free trade in England and among the "second-comers" on the Continent; the *Zollverein*; the role of the state in industrialization; the historical relations between agriculture and industry; balanced versus unbalanced growth; and industrialization and poverty.

Francesco Caracciolo attempts to show how the

"second-comers" reacted to the English paradigm of industrialization and how the observed response in each case was conditioned by social, intellectual, and economic background factors. He also studies the evolution of free-trade doctrines—Smith, Ricardo, Marx, Mill, Say, Rodbertus, and so on—and shows how they developed out of particular historical contexts. Free trade, he demonstrates, was a middle-class idea in England but an aristocratic one on the Continent, while protection was an aristocratic idea in England but a middle-class one in France, Germany, Italy, and elsewhere. The late Harry Johnson put it succinctly when he observed that states with increasing competitiveness in world markets in the nineteenth century were moving toward free trade, whereas states with declining competitiveness were moving away from it. Caracciolo, however, pays little attention to supply-and-demand factors in the process of industrialization—population change, prices, production, consumption, savings and investment, and so forth.

The author has read widely in his subject matter. Much of the research was carried out in Germany, and he offers many original and valuable insights. The difficulty with the study arises from the fact that our knowledge of the economic structure of many of the "second-comer" countries is rather sketchy and we know very little about the ways in which growth is transmitted from developed to developing regions. For Caracciolo, free trade was the most important factor in economic growth, but it was in fact only one of a large number of factors. Trade played a limited role in the economic growth of most nations until very late in the nineteenth century. In this respect, equating industrialization with the rise of free-trade doctrines, he greatly oversimplifies. Rolf Dumke has argued that the *Zollverein* retarded industrialization in Germany, and Paul Bairoch has recently shown that the "second-comers" achieved their highest growth rates in decades of high tariffs during the nineteenth century. Caracciolo has chosen an important problem to study, but one that does not lend itself to easy solutions.

IRA A. GLAZIER
Temple University

CHARLES REYNOLDS. *Modes of Imperialism*. New York: St. Martin's Press. 1981. Pp. viii, 263. \$27.50.

Historians and others have long understood that there are various kinds of imperialism. In this brief and elegant volume, Charles Reynolds presents a critical discussion of four of the more common modes—political imperialism, economic imperialism, ideological imperialism, and sociobiological imperialism. He chooses for test cases the postwar

arms race, the multinational corporation and national autonomy, Hitler's ideology and foreign policy, and Japanese imperialism in the 1930s. His attack is basically epistemological.

He finds that the power-security explanation of imperialism in world politics is inadequate for a number of reasons. Being essentially a conceptual scheme, in comparison with the other modes of imperialism, it is diffuse, ambiguous, and quite general. It defines concepts within the scheme, which really does not move the question further forward, and its interpretation is logical rather than empirical. This leads the author to conclude that the form of imperialism postulated by the power-security hypothesis is essentially a tautology.

Another theory of imperialism, repeated endlessly, is the economic one. But this theory cannot explain changes in the world market or the structure of production and investment except by rationalization after the fact. There is no way one can test Marxist or neo-Marxist theories of imperialism; actually such theories are political in nature and lack all objective standards. The disparate nature of Marxist argument over the nature of capitalist development is an indication of this weakness. There can be no genuine agreement or disagreement because there are no clear criteria of evaluation. The constant revision and reinterpretation of the Marxist line bespeaks an ideology, not a mode of explanation.

In the mind of the Marxist, Hitler's actions were imperialist because they were the actions of a capitalist agent. In relating theory to practice the author finds that, although National Socialism is an imperialist ideology, Hitler's actions were not imperialist. Notions of racial purity entail specific conditions for success of the program. Almost all of Hitler's actions can be explained as an attempt to create those conditions in which his ideology would work. He continually made a distinction between what was immediately necessary and the creation of a European empire for Germany.

The simple reduction of the sociobiologists' thesis that aggression equals expansion is held to be inherently nonexplanatory. To try to explain imperialism by reference to the biological nature of man merely transfers the problem of explanation from one context to another; it remains to be explained what aggression is, even if man's nature decrees it.

Each of the above theories has serious limitations. In fact, the author might have argued on the same grounds that no general theory of imperialism is possible. The central objection to these theories is that they do not advance the knowledge of human actions, but instead reformulate what we already know. The propositions derived from the empirical data contain a certain nugatory vicious circle. But we can be satisfied with a form of action tied to specific

individuals and situations. In other words, imperialism is best understood in relation to a specific time and place. The conclusions of this discussion are not new, but the approach is fresh. Almost any library would do well to include it in its collection.

MARK NAIDIS
Northridge, California

TONY SMITH. *The Pattern of Imperialism: The United States, Great Britain, and the Late-Industrializing World since 1815*. New York: Cambridge University Press. 1981. Pp. xi, 308. Cloth \$29.50, paper \$10.95.

The author of this essay-polemic, Tony Smith, is a political scientist at Tufts University who previously published a book called *The French Stake in Algeria*. Here he proposes to try for perspectives on the big subject of imperialism, raising moral issues as he goes along but eschewing both the emotional and the fussily particular and making use of "comparative history" (p. 11). Such an approach, he believes, can contribute to the dialogue through generalizations drawn from empirical analysis of representative instances. His chapters are on the dynamics of imperialism from 1815 to 1914, the impact of the phenomena, decolonization (by Britain and France, despite the subtitle), the American century, and American imperialism in the 1980s. Sources mentioned in notes place more emphasis on earlier essays and analytical studies of topics such as trade and politics than on monographs providing close-up views of alien rule. Predictably we hear about Hobson, Lenin, Gallagher and Robinson, Lewis, and Wallerstein. Less interesting, apparently, are such knowledgeable metropolitans as Macaulay, not to mention helpful specialists like Holden Furber. Imperialism is defined as domination that keeps client peoples from doing whatever they would do in its absence, although this is so heavily qualified that, in effect, virtually all forms of overseas activity are included. We end with a discussion of geopolitics in the large, some of it involving imperial rule and some not. Although China and the Middle East are cited, imperialism's field is spoken of as "the South" (p. 7 and elsewhere).

The world map facing page 3 is a curiosity that turns out to be symbolic. Trans-Jordan is abolished, as are the Hadhramaut and the Gulf dependencies; only one of the three High Commission Territories is included; North Borneo is lumped in with Sarawak; Malaya is called the "Straits Settlements"; the Gilbert Islands are labeled, but not the Ellices. In the text we learn that Britain was far more "immune" (p. 4) to the blandishments of private interests than the United States was (Smith might like to have a look at the doings of the Malayan agency houses in London); Britain's advances in eighteenth-century

India are attributable to superior military power; Austro-Hungarian territories in the Balkans are called "mandates" (p. 36) and Malayan units are "Princely States" (p. 52); the Mutiny showed that India was strongly resistant to change.

More worrisome is the vacuity of the book's findings and conclusions, if, indeed, those words are appropriate. Repeatedly the obvious is presented as startling revelation: domination originated when European power confronted tropical weakness and ended when this changed; since the turn of the century Washington has tried to advance its interests in the tropics through relations with governments there.

Lessons for the future? "There is nothing foreordained either in the domestic structure of the United States or in the international configuration that will determine how American policy should be conducted" (p. 233).

ROBERT HEUSSLER
Strafford, Vermont

MELVIN RADER. *Marx's Interpretation of History*. New York: Oxford University Press. 1979. Pp. xxiii, 242. Cloth \$12.95, paper \$4.95.

Melvin Rader undertakes to clarify Marx's thinking about history in a way that does justice to its richness and complexity. Rader argues that Marx's interpretation of history was a synthesis of diverse currents of thought and that Marx implicitly employed three different models: base and superstructure, organic unity, and dialectical development. The third model is not given independent treatment but is incorporated into the other two, which are elaborated at length in the first three chapters of the book. Rader's central thesis is that despite their apparent inconsistency it is possible to reconcile the base-superstructure and organic models in reconstructing Marx's view of history.

The model of base and superstructure is familiar, but Rader finds it simplistic and misleading as a guide to Marx. He contends that such basic Marxist concepts as "productive forces" and "relations of production" cannot be sharply distinguished and that science, education, property, law, and the state cannot plausibly be assigned exclusively to either base or superstructure. The organic model of society as an integrated totality, however, avoids the implicit reductionism of the base and superstructure model. But since organic unity is compatible with hierarchical relations of domination and subordination, the second model can still be true to Marx's thesis that the mode of production plays the determining role in society, which is the central claim of the base-superstructure model.

Rader's fourth chapter concerns Marx's use of

abstraction in theorizing and his objections to modern society's abstraction (or separation) of the individual from the (potential) organic unity of the human community. In this latter respect, Rader situates Marx in the "expressivist" tradition of Hegel, Feuerbach, and Schiller. The fifth and final chapter focuses on Marx's organic conception of historical crisis and revolutionary transformation. Interestingly, Rader contends that the familiar conflict between productive forces and relations of production must be matched in Marx's view by an "inner dialectic" of human powers and needs prior to the ushering in of a new historical epoch.

Rader's discussion of this inner dialectic is characteristic of his interpretation of Marx, which leans heavily on Marx's early philosophical manuscripts. His Marx offers us more of a vision of history than an empirical research program. Indeed, the different models that Rader identifies are little more than metaphors, and the organic totality interpretation he favors seems compatible with almost anything happening in history.

In this regard, Rader's book contrasts unfavorably with G. A. Cohen's *Karl Marx's Theory of History: A Defence* (1979). By elaborating with great skill just the sort of base-superstructure theory that Rader dismisses, Cohen undermines much of the persuasiveness of Rader's approach. Still, Rader's book is readable, humane, and occasionally insightful as he ranges far and wide—from Plato to Buber—in his effort to shed light on Marx's thought.

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FRED E. SCHRADER. *Restauration und Revolution: Die Vorarbeiten zum Kapital von Karl Marx in seinen Studienheften, 1850–1858*. Hildesheim: Gerstenberg, 1980. Pp. 285.

A remarkable feature of scholarship on Marx is the enormous interest aroused by his unpublished manuscripts. For this there are several reasons. A string of uncompleted works, whose titles are *Capital*, the *Economic and Philosophic Manuscripts of 1844*, and the *Grundrisse*, is enough to draw many a hungry researcher into the still-unexplored bottom drawers of Marx's desk. The widespread desire to discover a "real" Marx uncorrupted by the inadequacies and distortions of his votaries and interpreters has heightened the interest and impact of each previously unknown text. It is therefore surprising that until now no one has studied the notebooks Marx compiled on economics during the 1850s, in which materials later used in the *Grundrisse* and *Capital* were assembled. (They are available at the International Institute for Social History in Amsterdam.) Or rather it would be surprising were it not

for the well-known difficulties of deciphering Marx's extraordinary handwriting, a script so nearly illegible that one suspects it served Marx as much to hide his thought from others as to accomplish the "self-clarification" he often sought. Fred E. Schrader has surmounted the paleographic barrier and provided us with an extremely competent and useful account of just what those notebooks contain. Along the way he offers a series of germane and thought-provoking comments on the development of Marx's economic thinking, the political context and purpose of his theory, and his relationship to Hegel.

The notebooks offer no startling revelations on the scale of the other texts just mentioned. For the most part they treat questions about banking and currency reform, a question of considerable practical and theoretical interest during the nineteenth century. Nonetheless they are revealing links in the development of Marx's economic theory. Schrader discusses each notebook in turn. To show how their contents contributed to the *Grundrisse*, he also analyzes portions of that work in detail. Of particular interest is his demonstration that the analysis of commodities that became the starting point for both *A Contribution to the Critique of Political Economy* and *Capital* came to Marx only after the *Grundrisse* was completed. It served the need Marx recognized in the *Grundrisse* for finding a less idealistic presentation of his theory: the commodity was an elementary form of value rather than an abstract category determining its development.

Despite the value of Schrader's work, some of his views seem to me unacceptable. Schrader recognizes—indeed he makes clearer than before—that the theory of the *Grundrisse* that analyzed how surplus value arose out of the distinction between abstract and concrete labor was not formulated before 1857–58 and marked a wholly new stage in Marx's understanding of economic relationships. Yet Schrader's investment in the notebooks compiled before that date leads him to seek links between that theory and Marx's earlier ideas that—as he himself later admits—are merely superficial. Particularly questionable is the claim that a set of (previously published) comments on Ricardo in a manuscript of 1851 (Notebook VIII) constitute a partial approach to the theory of surplus value. Here the terms are still perfectly Ricardian, since profit arises from capitalists buying labor below its value, and no distinction is made between an increase in wealth (the total amount of goods) and a growth in value. Setting out from the notebooks of the 1850s, Schrader does not sufficiently see how much they still belong to the universe of *The Poverty of Philosophy*, a world very different from that of *Capital*. His willingness to take seriously the notion that Marx ceased work on economics after 1851

primarily because he could not find a publisher rests on a significant underestimation of the theoretical difficulties Marx then faced. A better understanding of how Marx's economic views developed requires a clearer recognition of how puzzling and labyrinthine the 1850s seemed to Marx and how deeply he had to reconsider his own earlier ideas before he could come to terms with the more stable and mysterious world of capitalism that emerged from the ebbing revolutionary tide of 1848.

JERROLD SEIGEL
Princeton University

ATHAR HUSSAIN and KEITH TRIBE. *Marxism and the Agrarian Question*. Volume 1, *German Social Democracy and the Peasantry, 1890–1907*. Atlantic Highlands, N.J.: Humanities Press. 1981. Pp. x, 152. \$22.50.

ATHAR HUSSAIN and KEITH TRIBE. *Marxism and the Agrarian Question*. Volume 2, *Russian Marxism and the Peasantry, 1861–1930*. Atlantic Highlands, N.J.: Humanities Press. 1981. Pp. ix, 167. \$22.50.

The peasant was a source of embarrassment to many Marxists because he did not fit neatly into Marx's categories of social classes. Marx tended to see the peasantry as a holding company of social groups that included the rural counterparts of the bourgeoisie and the proletariat. Denial of group identity or group consciousness to the peasantry often crippled Marxist social analysis and planning for the revolution, at least before Lenin.

Athar Hussain and Keith Tribe, the authors of these two interesting descriptions of Marxist theory on the agrarian question, try to escape from this conventional wisdom without jettisoning Marx. They do this by seeing Marx's work as a fund of insights rather than holy scripture. It is true, they admit, that Marxists have had little to say about the peasantry, but they have had a great deal to say about the agrarian question in the historical, political, and economic setting in which it works. The authors try to avoid easy generalizations about "the peasantry," "the state," or "the Marxists." Instead, they suggest, there is much insight to be found in Marxist analysis of agrarian problems in particular times and places. Therefore, they do not try in these two volumes to give a history of the agrarian question in German and Russian Marxism but rather focus on some of the major issues for Marxism that grew out of the agrarian questions in each area, basing their own research on easily available primary and secondary sources in English and German.

By narrowing their range the authors achieve a sharper focus, giving themselves the opportunity, for example, to compare Kautsky's *Agrarfrage* with Lenin's *Development of Capitalism in Russia*. Lenin's

originality is seen in his detailed analysis of the Russian peasantry, from which he derives a taxonomy of that social class. The authors stoutly deny that Lenin's strategy of playing off one stratum against another was exploitation, because he correctly perceived that any other "solution," like that posed by the Russian Socialist Revolutionary party, would lead to capitalism and the victory of the rural bourgeoisie in the countryside. The authors achieve some interesting insights into agrarian politics by treating the state as a mediator between interest groups, rather than as a single corporate personality.

Despite some originality in their approach, the authors frequently criticize others, like Carr, Lewin, and Shanin, who have provided much more impressive documentation for their views. Although the two books provide a convenient sketch of the development of Marxist views on the agrarian question in pre-World War I Germany and revolutionary Russia, in the latter case the study suffers from a lack of Russian-language sources. On balance, the two books have considerable value in summarizing and evaluating current studies of the subject and in pointing the way to future reassessments.

GEORGE JACKSON
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LOUISE A. TILLY and CHARLES TILLY, editors. *Class Conflict and Collective Action*. (New Approaches to Social Science History, number 1.) Beverly Hills, Calif.: Sage. 1981. Pp. 260. Cloth \$20.00, paper \$9.95.

This is a collection of chapters by eight different authors, with Charles Tilly contributing a brief introduction and a chapter on disturbances in late eighteenth-century London, Charleston, and Paris, and Louis A. Tilly furnishing a brief conclusion and a chapter on forms of female collective activity in France between 1870 and 1914. These express the familiar Tilly thesis on fundamental changes in the nature of collective action because of such large-scale structural changes as capital concentration, growth of wage labor, and centralization and bureaucratization of states. The various contributions are seen as part of an extensive and coordinated program of research on collective action and the impact of capitalism and large-scale organization on workers' lives and their capacity to control them.

Frank Munger deals with Lancashire popular protest for the period 1750–1830, and Brian Brown studies Chartism and the general strike there in 1842. Both share the Tillys' rejection of attribution of protest to stress and breakdown consequent on early industrialization, and both emphasize political repression as giving a rationality to workers' de-

mands for political power. Munger also stresses the importance of middle-class political mobilization. Wayne Te Brake studies the village of Barthmen in the eastern Netherlands during the Dutch patriot revolution of 1787 and relates conflicts over land to patriot politics. Samuel Cohn's study is of discipline among British railway navvies, based mainly on the 1846 Select Committee on Railway Labourers. Judy Reardon's brief chapter looks at tension between French and Belgian workers in nineteenth-century Roubaix. Laura Frader deals with vineyard workers in Coursan, a village in the Aude, 1860–1913, relating changes in the industry and labor process to the rise of labor unions distinct from petty-bourgeois movements.

On the whole the contributions put forward interesting arguments, but in a rather general and sometimes speculative way, and do not establish their points through full discussion or provision of evidence. Despite the Tillys' stress on the importance of taking the definitions and beliefs of the participants in collective action seriously, this is not on the whole done in the contributions, such assessments remaining at a somewhat abstract level. Nor is there any systematic study of capital concentration, proletarianization, or city societies, which tend to be involved in a rather generalized and external way. To some extent the chapters by Te Brake and Frader are exceptions, but these deal with single villages. The contributions are thus not definitive studies and do not fulfill the research program outlined by the Tillys. They are best seen as provisional statements in connection with an important and ongoing collective research project.

IORWERTH PROTHERO
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KARL-HEINZ KLÄR. *Der Zusammenbruch der Zweiten Internationale*. New York: Campus. 1981. Pp. 365. DM 48.

It is a commonplace among the historically educated that, in the struggle for the soul of the international proletariat, nationalism won and the international class struggle lost, and that the struggle was decided if not at the time of the outbreak of World War I, then certainly before the war was a year old. The story is inordinately complicated. Karl-Heinz Klär has with great diligence traced not only the prewar history of the collapse of antiwar internationalism but also the intranational conflicts within the socialist parties in the major countries of Europe. He has further shown how the triumph of the right-wing socialists who collaborated with the war effort, on both sides of the fence, was to lead to a final split five years after the end of the war, but that event will surely be given clearer attention in later installments.

Studying this gripping book one is heartened to learn that two more volumes are in preparation, so that a treatment of the three years between the outbreak of war and the October Revolution in Russia, and the years beyond that point, awaits the fascinated reader. For those of us who are particularly interested in the so-called nationality problem within the Austro-Hungarian empire and its relationship to international socialism, such a prospect is indeed attractive. Incidentally, since Klär seems to view Karl Renner, who gave so much thought to that problem, as harboring "reactionary" notions (p. 72). This may be one of the few exaggerations in an otherwise flawlessly balanced book by an author who says about himself that, although he refuses to engage in the quarrels of what in America may colloquially be called "Monday morning quarterback," he cannot deny a certain sadness (*Betroffenheit*) at the tragedy of the European working classes. Klär organizes his work in two sections: the first deals with the confusion the threat of a general war caused in the socialist parties with regard to their international commitment in an age of rising nationalism. Here the pious hope in congress after congress that somehow war will be averted, so that the problem of what to do if it comes will go away by itself, emerges among all the major European labor movements. Klär, by the way, is impeccably informed about the intraparty feuds within each of the important countries with socialist movements of any consequence. He shows how these intraparty differences manifested themselves in the meetings with "fraternal parties" from other countries before 1914 and how the resulting obfuscation contributed to the breakdown of a solid, internationally oriented socialist movement, even before June 28, 1914, and the ensuing July crisis.

The final blow at whatever Continent-wide solidarity of the organized working classes there may have been came with the war and the appropriations bills, supported by the socialists who "forgot their pacifism in the general frenzy of patriotism." Klär explains carefully the nature, source, and consequence of the *Burgfriede*, not only in Germany but also in France and elsewhere. Whenever he mentions Russia, one is reminded of the history of the past generation, which, though under very different auspices, has shown the moral if not the political and military victory of nationalism over communism in Eastern Europe. But whether this situation also triggers a kind of *Betroffenheit* must be left to each student of recent European history.

ROBERT SCHWARZ
Florida Atlantic University

ADAM WESTOBY. *Communism since World War II*. New York: St. Martin's Press. 1981. Pp. xiii, 514. \$32.50.

One can only admire the audacity of Adam Westoby for venturing to survey in some depth a subject that less intrepid scholars might well dismiss as unmanageable. With over a hundred communist parties to choose from, including more than a dozen enjoying political rule, the scope of the book (and much of its content) is truly global in nature and even approaches the encyclopedic. Thankfully the narrative successfully avoids the bland detachment that tends to characterize works of reference. Indeed, the text is lively and contentious; the style is brisk and vigorous, if often cluttered and deficient in grace and subtlety; and the documentation is impressive (more than a hundred pages of reference notes and bibliography), though dominated by secondary literature. Nearly a third of the book is devoted to theoretical aspects of communism ("The Problem of Stalinism," and so on), a section that is occasionally provocative but abstract and tedious compared to the avowedly historical chapters.

The interpretation is a refreshing departure from the familiar homilies of anticommunist rhetoric. Like most Western scholars, Westoby is no admirer of communism (its political format is labeled a "bureaucratic nationalized state"). Yet he writes from the perspective of the radical but presumably democratic left, with an implicit distaste for industrial capitalism, and he spices his volume with jabs at "American imperialism" and other pejorative remarks. His sympathies lie with the downtrodden and impoverished, and there is an eloquence not apparent elsewhere in his description of peasant conditions, especially those of rural China in the 1930s and 1940s. He suggests in one striking passage that in undeveloped countries the bureaucratic rule of communism is the lesser evil compared to a kind of Social Darwinist society run amuck: "Many things will be hidden from the visitor to Shanghai or Hanoi by the political regime, but one thing it does not need to hide is a vast destitute population, driven from the countryside, eking out an existence in casual work, peddling, beggary, petty crime and prostitution, such as fill the shanty towns round Djakarta or Rio de Janeiro" (p. 354).

A veritable cornucopia of party history, extending even to Southeast Asia and South America, the work has, inevitably, flaws and deficiencies. Take the vexed question of space allotment. Should half of a page be devoted to the Portuguese party and that of Spain be virtually ignored? Does Ecuador merit a paragraph while Mexico languishes? Surely the ruling party of the Mongolian People's Republic, albeit a Soviet satellite, deserves a mention? And what of less trivial issues, such as international terrorism and its alleged ties with the Soviet bloc? Nor would a discussion of overpopulation, a specter from which communist countries are not immune, be taken amiss. The factual reliability assays at a high level

considering the staggering amount of information assembled by a single hand. (But did President Roosevelt really prefer fascism to communism? And who assigned Czechoslovakia to the Soviet sphere at Yalta and Potsdam?) Strictures aside, this is a unique and possibly indispensable compendium for students of world communism.

ROBERT D. WARTH
University of Kentucky

VIVIAN C. FOX and MARTIN H. QUITT. *Loving, Parenting, and Dying: The Family Cycle in England and America, Past and Present*. New York: Psychohistory Press. 1981. Pp. v, 488. Cloth \$38.50, paper \$11.95.

Vivian C. Fox and Martin H. Quitt have continued the Psychohistory Press's tradition for avant-garde work, but they have taken us an important step further by focusing on the individual and family life cycle. In format this work resembles Lloyd de Mause's earlier *The History of Childhood*, but by utilizing a more confined geographical and historical scope and a more useful conceptual framework this book is likely to receive a more serious reception. Avoiding the tendency for heavy Freudian hypothesizing, Fox and Quitt meticulously synthesize much of the recent work in family history and place their analysis in the midst of the family developmental framework.

Fox and Quitt overlook recent important modifications by scholars such as Glen Elder and Peter Uhlenberg as well as some of the intrinsic problems of the developmental framework, but they do provide us with a bounteous repast of historical materials. Restricting themselves to the early modern period in England and America, Fox and Quitt begin with a delectable entrée of past versus present. Few historians have so creatively attempted this challenge.

As noted earlier, the book is unconventional in that the authors begin with a long speculative essay (ninety pages including detailed explanatory footnotes), laying out their six "stages" of the family developmental cycle: courtship to marriage formation, preparenthood, childbearing, childrearing, post-childrearing, and spouse-loss. Forty-five short articles written by well-regarded authors on the same stages in specific times and places constitute the second and far longer portion of the book.

In the process of reading this work, two central questions arose. First, why did the author-editors not eliminate the articles and extend their survey of English and American family life? Most of the articles are abbreviated versions of works that can be found elsewhere in their entirety. We may answer this question by noting that Fox and Quitt have done us a considerable service by bringing together

hard-to-find materials and placing them within a central conceptual context.

A second point one might raise is why did the author-editors include all this material on early modern England and America and then jump to contemporary America? We may answer on the pragmatic grounds that Fox and Quitt are specialists in the former areas as well as codirectors of the Center of Family Studies at Boston State College. They represent historical specialists planted firmly in the midst of the current American family scene. Eventually, we hope that they will go further in terms of *linking* past and present from the antebellum American period through World War II. This latter time span may well hold important clues to contemporary American family life. Fox and Quitt have provided us with an exceedingly important synthesis of the early modern period and a creative comparison with the present, but the investigation of the evolution of present trends is still a major unfinished agenda item.

ROBERT S. PICKETT
Syracuse University

ANTHONY SUTCLIFFE. *Towards the Planned City: Germany, Britain, the United States, and France, 1780–1914*. New York: St. Martin's Press, 1981. Pp. x, 230. \$25.00.

In this concise, thoughtful, and ambitious book, Anthony Sutcliffe argues that the essential theory and practice of modern urban planning emerged in the years 1890–1914. His emphasis on this period at first seems eccentric. These years saw nothing to compare in scale with either the rebuilding of Paris from 1850 to 1870 or the massive post-1918 reconstruction projects. Nor were there famous and controversial figures like Baron Haussmann or Le Corbusier to focus public attention and professional debate. The pre-1914 planning “profession” consisted of a miscellaneous group of reformers, bureaucrats, architects, and journalists. The universities, needless to say, had barely heard of the subject.

Yet it is precisely Sutcliffe's achievement to see in these “arcane” years “the most important episode in the whole history of public intervention in the environment” (p. 202). There were no massive projects, but there was a new understanding that successful planning must combine the hitherto separate fields of housing reform, transportation policy, suburban extensions, zoning, and aesthetics into a coherent and continuing strategy for urban growth. There were no towering geniuses, but there were many creative innovators whose collective achievement Sutcliffe records. His remarkable command of the primary sources and his expert use of recent scholarship have enabled him to bring together this

complex material into a convincing and original synthesis.

The heart of the book is a series of chapters on planning in each of the most advanced nations: Germany, Britain, the United States, and France. Although the book's subtitle gives 1780 as the starting point, the century before 1880 is passed over very quickly, and the chapters concentrate on the years before World War I. Sutcliffe presents clear, detailed narratives that, taken together, constitute the best short account of planning history in the respective nations from 1890 to 1914.

But Sutcliffe is also concerned with a problem that goes beyond national history. To what extent did planning emerge out of the separate political and cultural institutions of each nation, and to what extent was planning a truly international movement? Sutcliffe does not underestimate national differences, but his wide knowledge and comparative approach have led him to uncover far more international contact than planning historians had realized.

As he shows, each nation brought to the emerging “international style” its own characteristic achievements. The United States contributed urban park systems and “City Beautiful” aesthetics; France, the Beaux-Arts tradition of large-scale order; Germany, the wealth of administrative powers and techniques; and Britain, the “Garden City” concept of planned decentralization. The foundations for twentieth-century planning, Sutcliffe concludes, “had been laid before 1914; there have been no radically new departures since” (p. 202).

At the end of the book Sutcliffe observes that the actual achievements of twentieth-century planning have been “disappointing” after the theoretical ferment and great expectations of 1890–1914. This is a notable understatement. Sutcliffe never directly confronts the question of whether defects in the pre-1914 foundations might explain some of our present problems. In his account, the basic evolution from urban *laissez faire* “towards the planned city” seems as necessary and desirable as the development of better sewers.

Yet in the evidence he presents one can clearly discern the origins of many later planning disasters. These are perhaps most apparent in the various pre-1914 “grand designs,” of which Daniel Burnham's plan for Chicago (1909) was surely the most grandiose. Such plans seemed to embody the ideal of the comprehensively planned city; they “fire[d] men's blood,” in Burnham's phrase; they also anticipated the brutal uniformity and inhuman scale that would disfigure so much of twentieth-century planning.

As Burnham predicted, the grand designs monopolized public attention—then and now. But Sutcliffe's detailed research also provides many ex-

amples of another, less assertive approach to pre-1914 planning exemplified by Raymond Unwin's designs for Hampstead Garden Suburb (begun 1906). This "small-scale tradition" emphasized variety, individuality, a sympathetic approach to the terrain, and a respect for traditional forms.

The grand designs have run their course, but their small-scale counterparts—many of which Sutcliffe has rescued from obscurity—still seem well worth remembering and preserving. Perhaps Sutcliffe's most important service in this book has been to recover some of the more modest and humane aspirations that once shaped the quest for the planned city.

ROBERT FISHMAN
Rutgers University

JORMA AHVENAINEN. *The Far Eastern Telegraphs: The History of Telegraphic Communications between the Far East, Europe, and America before the First World War*. (Suomalaisen Tiedeakatemia Toimituksia, series B, number 216.) Helsinki: Suomalainen Tiedeakatemia. 1981. Pp. 226.

In the half-century before World War I Europe and America were linked to China and Japan by electric telegraph. The apparent simplicity of this statement belies the complicated nature of the process, in which not only Western companies and entrepreneurs but also governments, commercial communities, and Oriental politicians were involved. In *The Far Eastern Telegraphs* Jorma Ahvenainen has compiled a detailed narrative account of this important development. As the author states in the preface, he has not attempted to write a history of the individual firms that sought profit and prestige in the East. Rather he has addressed the more difficult and more useful task of presenting "a survey in which the political and economic aspects of the subject are considered in their relation to each other" (p. 7).

In seeking to accomplish this goal, Ahvenainen divides his book into four main sections. First, he traces the efforts of two firms—the Eastern Extension Company of England and the Great Northern Company of Denmark—to build telegraph routes to China via India and Russia respectively. Then he discusses the promotion and establishment of internal telegraph networks by Chinese and Japanese authorities and the Western companies' reactions to these developments. In the third section the elaborate maneuvers of the 1880s and 1890s, when firms searched for more secure markets, business groups demanded lower tariffs, and governments were forced to adjudicate among competing interests, are covered. Finally, the growing involvement of the United States and the origins of the Pacific cable are brought into perspective.

There is much to be said for Ahvenainen's scholarly treatment of his subject. He has based his account on an extensive use of archives in four national capitals. Moreover, he is very much aware of the importance of indigenous social forces in shaping technological change. He emphasizes, for example, the difference between the early Japanese interest in the telegraph and the more hesitant Chinese approach. The author also sets the story in the wider context of imperial history, discussing the impact of the Boxer Rebellion and the Spanish American War. Indeed, one of the main themes of the book is the paramount role that politics came to play. Ahvenainen concludes that by the turn of the century the telegraph "had become an instrument of world politics, and cost-effectiveness was no longer the first question" (p. 212).

Unfortunately the value of this work is diminished by several weaknesses. Much of the book is written in the old style of diplomatic history—what one clerk said to another. Too often paragraphs consist of dry summaries of letters and memoranda. As a result the figures who influenced the history of the telegraph—whether businessmen such as Sir John Pender and C. F. Tietgen or government officials such as Sir John Lamb—remain two-dimensional. Furthermore, the author might have raised his intellectual sights to consider the relationship between communication by telegraph and the similar issues of conveyance of mail by steamship. Perhaps the most serious flaw is the failure to explain clearly what difference the telegraph made. Without more information on the volume of traffic and the specific nature of the clientele, one cannot fully understand the telegraph's role. Nonetheless, this workmanlike volume will be of interest to imperial historians and to students of technological development.

CHARLES R. PERRY
University of the South

MICHAEL I. HANDEL. *The Diplomacy of Surprise: Hitler, Nixon, Sadat*. (Harvard Studies in International Affairs, number 44.) Cambridge: Harvard University Center for International Affairs. 1981. Pp. xi, 369. Cloth \$22.50, paper \$11.95.

When the diplomatic horizons look bleak, when other governments are jostling you too hard, when you own bureaucrats and even some of your close advisers seem to be minding their own accounts rather than yours, break free from the crowd with a *fait accompli*. That is Michael I. Handel's advice to statesmen and would-be statesmen. It is not new advice. Surprise—confounding your rivals and sometimes even your friends with new "facts"—has always been a tool of diplomacy. But diplomatic

surprise in the middle and late twentieth century, amid big bureaucracies, importuning legislatures, powerful intelligence agencies, and prying media is somewhat more difficult to achieve than it was in, say, sixteenth-century Italy. That is why Handel's account of how Adolf Hitler, Richard Nixon, and Anwar Sadat managed surprises that fundamentally altered the international political landscape is such interesting reading.

Handel has written detailed narrative accounts of five diplomatic surprises, two each by Hitler and Sadat and one by Nixon. They are Hitler's remilitarization of the Rhineland in 1936 and his pact with Stalin in 1939, Sadat's expulsion of his Soviet military advisers in 1972 and his journey to Jerusalem in 1977, and Nixon's opening of relations with China in 1971. In each of these instances (although considerably more in some than in others) there were advance tremors of the earthquake to come. Why were they for the most part undetected? And if detected, why was their significance generally not perceived? Intelligence failure is the complement of surprise. Much of Handel's five case studies is given over to discussion of what clues reached what observers when, and why they made so little of them. Handel's conclusions, either about how diplomatic surprises come about or why they so often succeed, are scarcely startling—there is a large existing literature on intelligence failure in particular—but they are thoughtfully articulated and they deserve study by those who would seek to avoid being surprised.

Handel's cases are also worth reading. They are excellent short narrative accounts of the episodes they retell. In each instance they are based on very wide reading of published primary and secondary materials, some of them quite obscure. Handel asks the same questions about each of his cases: Why did a leader choose surprise? How was surprise achieved? Why was it not detected? What were its immediate and long-range benefits and costs? Handel's narratives are accompanied by quite detailed chronologies, and his summary chapters include some quite helpful tables in which the various surprises he chronicles are displayed comparatively against a number of situational variables. Alas, the book contains no index—a serious omission in a volume so long and so relatively dense.

RICHARD H. ULLMAN
Princeton University

ANCIENT

MICHAEL GAGARIN. *Drakon and Early Athenian Homicide Law*. (Yale Classical Monographs, number 3.) New Haven: Yale University Press. 1981. Pp. xvii, 175. \$20.00.

Building on Ronald Stroud's exemplary study, *Drakon's Law on Homicide* (1968), Michael Gagarin proposes a new interpretation of an Athenian inscription (*I.G.* i³ 104) from the fifth century B.C. containing a portion of Draco's law code. Gagarin accepts Stroud's arguments that the inscribed text is an essentially accurate copy of a document of the late seventh century but denies that it is only the section dealing with involuntary homicide. Rather it is "the basic statement of the law on all homicide, intentional and unintentional. In other words Drakon wrote no separate law on intentional homicide but by beginning his law with the statement 'Even if a man not intentionally kills another, he is exiled' he meant to state the law on all homicide" (p. 101). The opening sentence, therefore, is an ellipsis that implies that exile was the penalty for both intentional and unintentional homicide.

This is the central thesis of Gagarin's monograph. It avoids some difficulties that have beset earlier interpretations and is supported by thoughtful and learned arguments but encounters difficulties of its own. It requires us to assume that Draco, a formidable stylist according to Gagarin, expressed himself with an ellipsis that "is extreme and . . . unparalleled" (p. 110). We must also assume that Draco here departed from his normal preference for capital punishment. Although Draco's laws, even on minor matters, were said to have been "written in blood," in Gagarin's view he allowed exile to be the penalty for murder. This leads to further dubious contentions, including the assertion that "in Drakon's time capital punishment carried out by the polis did not exist" (p. 119). Most damaging to the thesis of the monograph, however, is its inability to account for ancient quotations and paraphrases of a section of the law dealing with intentional homicide (Demosthenes 20. 158; 23. 51; Compare Gagarin, p. 66).

Gagarin's exhaustive lexical studies of the opening words strengthen the case for taking them as "even if," rather than as a connective to some now lost portion of the code. This is used to support Gagarin's thesis that exile was the penalty for both intentional and unintentional slayings. But does the sentence specify a *penalty* or a *procedure*? Does its main verb state the punishment or introduce regulations of the sort summarized in Demosthenes 23. 72: "The person who is caught in involuntary homicide shall in a certain specified time leave the country on a specified route and go into exile until he is pardoned by some of the kin of the deceased." If this suggestion is correct, the slayer, even if his deed was not intended, cannot stay in the city but must withdraw until a process of pardon carefully regulated by the state allows him to return. Many of the later provisions of the law accord protection to the slayer who follows these procedures. A later

section of the law would have then contained procedures for cases of intentional homicide, as Stroud plausibly argues.

Gagarin's discussions illuminate many features of Greek law and society and make this book a useful companion in the study of Athenian history, even if its central thesis cannot be accepted.

W. R. CONNOR
Princeton University

H. B. GOTTSCHALK. *Heraclides of Pontus*. New York: Clarendon Press of Oxford University Press. 1980. Pp. x, 178. \$37.50.

The Academy of Plato is a perennial subject of scholarly interest, and one motive for inquiry into this unique educational establishment is the quality of the pupils trained there. Of all its pupils none rivals the monumental genius of Aristotle. Over the past several years a Leeds University classicist has been engaged in a history of the Peripatetic school, and H. B. Gottschalk's *Heraclides of Pontus* is an offshoot of this larger endeavor. This slim but costly volume is a most impressive reconstruction, the first in English in this century and intended for an audience with some familiarity with Greek.

Although Heraclides and Aristotle spent nearly two decades under Plato's tutelage, we know very little about this popularizing Platonist. Our source material consists of 181 surviving fragments—none from the pen of Heraclides—drawn from doxographical citations. Employing sober judgment and restrained imagination, Gottschalk demonstrates Heraclides' innovative use of the dialogue format (of which forty-six titles survive). Much later Cicero would praise his writings for their literary merit, especially the imaginative use of exotic themes and Hellenic personages drawn largely from the archaic age. On more sobering matters present-day scholars are still intrigued by his distinctive atomist theory of matter and his astronomical hypotheses that seem to anticipate the later discoveries of Aristarchus and Kepler.

The first of seven chapters documents what is known of his life and activities, couched within a sensitive rendering of the mid-fourth-century cultural context. Next we turn to the one dialogue that allows for some detailing of his doctrines, the *Apotheosis of Empedocles*. Heraclides' indebtedness to the Pythagorean tradition is abundantly evident, and Gottschalk credits to Heraclides the origin of the parable of three lives in its modern dress. The range of his cosmological interests is documented, including the continuing debate about the manner in which atoms combine. Gottschalk's command of the secondary literature is impeccable.

Turning our attention from the microcosm to the

macrocosm, chapter 4 finds Heraclides' cosmology to be geocentric; he may have been original in combining the notion of a rotational earth with a systematic use of epicycles that figure prominently in the writings of Aristotle. Gottschalk stresses the importance of Heraclides' normative judgments to the later Hellenistic thinkers, however. On matters psychological, ethical, and religious we discover fundamental departures from Plato, and especially noteworthy is the omission of any reference to the theory of forms. As one might expect, Gottschalk encounters difficulties in assessing the part played by Heraclides in the philosophical controversies of his day. We do know that he became a student of the history of philosophy before Aristotle systematized the discipline. Yet many matters seem to defy explication, as when Cicero ranks Heraclides with Plato and Aristotle as political theoreticians. Given the sources at our disposal, Gottschalk is quite right to stress repeatedly how little we know and thereby to resist speculative excesses.

Two appendixes, a thorough bibliography, and the requisite indexes round off this excellent portrait of Heraclides. It was Heraclides' tendency to moralize and invoke divine intervention in human affairs, features for which he will be slighted. Yet in Gottschalk's judgment Heraclides was unable to weld his divergent interests into a coherent system because he lacked a unifying concept akin to Plato's forms. In the final analysis, his readers admired above all his command of illustrative material—the analogies, myths, metaphors, and interplay of fact and fancy. In the use of these models he redefined the nature and role of Platonic dialogue for his Hellenistic readers.

RONALD H. EPP
Memphis State University

KENNETH SACKS. *Polybius on the Writing of History*. (University of California Publications in Classical Studies, number 24.) Berkeley and Los Angeles: University of California Press. 1981. Pp. viii, 233. \$20.00.

This book is a painstaking study of Polybius's statements, found scattered through the *Histories*, concerning his view of proper historical techniques. Kenneth Sacks supposes that "Polybius developed perhaps independently of other historians, a historiographical vocabulary which he assumes is intelligible to the reader through definition and context" (p. 161). By necessity, the work is more philological than historical in character. Skillfully using the tools of literary and textual criticism, the author examines the meaning of the key terms and phrases that Polybius used in his historical arguments and definitions. Sacks succeeds because he translates them as

literally as possible, always within the context of Polybius's *Histories* and with minimal references to modern historiographical theories.

Since only about 30 percent of Polybius's text is extant, Sacks's first concern is to show that medieval excerptors of Polybius must have concentrated on methodologically important passages in which Polybius's thoughts on the proper approach to history might be viewed holistically (chap. 1). Approximately one-third of the entire volume (pp. 21–95) is an outline of book 12 of the *Histories*, which the author considers Polybius's "personal handbook" for the writing of historical narrative. Sacks argues persuasively that in this book, and within the frame of a bellicose attack on other historians, Polybius presents his complete thoughts on the requirements for writing errorless and realistic historical narrative. The primary requirement is a commitment to personal inquiry (*polypragmosyne*) and experience (*empeiria*, *autopatheia*, or *autourgia*). Sacks shows that as a secondary requirement Polybius connects knowledge—derived from personal involvement in the events—to a vague term, *emphasis*, which in the context of the *Histories* implies the transference of the historian's knowledge to the reader. Sacks analyzes the Polybian usage of the word in all its variegated meanings. He comes to the conclusion that Polybius applied emphasis and vividness according to his special historiographical or methodological needs (p. 40), based on the belief that a historian must be a man of action, a *politikos anér*, committed to a special ethos from which he can draw experience and knowledge. In this context the author discusses Polybius's genre, universal history (chap. 3), and Polybius's subject matter, *pragmatiké historia*, which Polybius does not define but indirectly shows that it should substantially, but not exclusively, contain political and military information (chap. 5). In chapter 4 Sacks pinpoints a governing purpose of Polybius's method: the advantages that Polybius promises will accrue to the reader from history written according to his methodology. Sacks concludes that the *Histories* contain the framework of Polybius's historiographical principles, which must be studied within the perspectives of Polybius's own age.

As promised in the introduction, the book provides a more complete picture of Polybius's method and should be considered an important addition to the considerable number of recent studies of the historian. The book will appeal to scholars of classical antiquities. One hopes that the author will expand this appeal in a future study by contrasting Polybius's historiographical principles with those of modern theorists.

G. J. SZEMLER
Loyola University of Chicago

HUNTER R. RAWLINGS III. *The Structure of Thucydides' History*. Princeton: Princeton University Press. 1981. Pp. xiv, 278. \$21.00.

This interesting book presents an interpretation of the *History* of Thucydides based on an analysis of the work's structure, and Hunter R. Rawlings III is admirably clear as to what he means by structure: selection, emphasis, and juxtaposition. These categories are splendid for revealing many of the indirect but powerful ways that Thucydides conveys his meaning. Rawlings's idea that Thucydides has carefully constructed his work to reveal ironies and relationships between earlier and later events is entirely persuasive and produces many valuable illuminations of the *History*. Unfortunately Rawlings has been captured by a notion of Thucydides' structural plan that is not supported by the evidence and is so rigid as to prevent him from seeing all that one can learn from a broader understanding of the structure of the *History*.

Rawlings's central thesis is that Thucydides conceived the Peloponnesian War to be composed of two wars, each of about ten years in length, the first being the Archidamian War (431–21 B.C.) and the second beginning in the winter of 415–14 and ending in 404 B.C. This "double vision" through which Thucydides sees the war, "this constant comparison and contrast of the events in the two wars, is the principal thematic regulator of Thucydides' work. . . . It may not be an exaggeration to say that this double vision is the wellspring of Thucydides' *History*" (pp. 5–6). Rawlings finds support for his opinion in 5. 25, where Thucydides speaks of conditions after the Peace of Nicias: "there was peace for those who accepted the treaty, but the Corinthians and some of the cities in the Peloponnese began to disturb the agreements, and immediately *another* disorder of the allies against Lacedaemon occurred. . . . For six years and ten months they held back from invading each other's territory, but outside [their territory] they did a great deal of harm to each other in an unstable truce. Then, however, forced to break the treaty made after the ten years, they *again* went into open war" (Translation, bracketed words, and italics by Rawlings). The emphasized words are taken by Rawlings to show that Thucydides "is thinking here of the second of two wars. This was the second time that a disorder had arisen in Sparta's alliance—the first was that of 432/431 when, also led by Corinth, Sparta's allies had come to her with complaints about Athenian aggression and thus instigated the first ten-year war. Now, Thucydides says, the process began anew. It was eventually to lead to a second ten-year war, though this time it would take six years and ten months for open war to arise from disorder" (p. 14).

This interpretation is forced and unpersuasive in several ways. First is the matter of translation. Rawlings's version: "and immediately *another* discord of the allies against Lacedaemon occurred" changes the order of Thucydides' words, which run as follows in the more accurate and neutral translation of F. C. Smith: "and at once other troubles also began between Lacedaemon and her allies." There is no need to imagine an intended connection between the defection of Sparta's allies after the Peace of Nicias with their behavior before the Peloponnesian War. The word *allê* ("other" or "another") is more likely contrasting the *tarachê* ("discord" or "trouble") recently ended between Athens and Sparta, with the new one, this time between Sparta and its allies. This leads to a second objection, that a comparison between the actions of Sparta's allies in 432–31 with those after 421 would certainly not be apt. After 421 the dissident allies broke their oaths and agreements by refusing to obey a majority vote of the Peloponnesian League, and by organizing and joining a separate league led by Sparta's bitter foe, Argos. Such actions deserve to be called a *tarachê*. In 432–31 the allies merely tried to persuade Sparta to help them in their grievances against Athens, as they had every right to do. There were no broken oaths, violated agreements, or hostile alliances, thus no *tarachê*. The parallel is not in Thucydides' *History* but must be supplied by Rawlings. Finally, most readers of the passage in question will conclude that when Thucydides says, almost immediately after the words quoted by Rawlings: "the war lasted twenty-seven years in all; and if anyone shall not deem it proper to include the intervening truce in the war, he will not judge aright" (Smith's translation), he was particularly trying to tell his readers that what he was writing about was one unitary war, not two, or several.

Rawlings's preoccupation with the idea of comparisons only between events in the putative two ten-year wars leads him to ignore other important structural comparisons and ironies, some of them well known and important. The stark juxtaposition of Pericles' Funeral Oration with the description of the great plague at Athens, a similarly dramatic placement of the Sicilian expedition immediately after the Melian Dialogue, the striking verbal similarities between Cleon's speech on Mytilene and Pericles' first speech, these are only a few examples of ironic and telling messages to be gleaned from structural analysis, though they occur within a single ten-year war or between the intermediate period and the "second war."

Happily, however, the failure of Rawlings's central thesis does not spoil the great merits of his book. There is no space here to list all the passages on which Rawlings sheds valuable light, but a few come quickly to mind. The famous digressions of Themis-

tocles, Pausanias, and the tyrannicides receive interesting and generally persuasive interpretations. The contrasts between pairs of generals and statesmen are sound and revealing. The thoughtful discussions of the relationships between human intelligence, chance, training, and power are among the best elements in this perceptive and learned book.

DONALD KAGAN
Yale University

FRIEDRICH-KARL KIENITZ. *Völker im Schatten: Die Gegenspieler der Griechen und Römer von 1200 v. Chr.–200 v. Chr.* (Beck'sche Sonderausgaben.) Munich: C. H. Beck. 1981. Pp. 364. DM 42.

Students of Marlborough's career are most likely to assess it from the vantage point of London or Paris; they stand far less often in Vienna, but if they do matters appear very differently. So too ancient historians commonly place themselves in Athens or Rome; if they move to Susa or Carthage they will assess interrelations with the Greeks or Romans in a much-altered light. Friedrich-Karl Kienitz, a specialist in Near Eastern history, has had the happy inspiration of seeking to look at a number of the neighbors of the classical powers for themselves and as far as possible from their own sources, which are now increasingly archaeological.

The execution of the project is alas very disappointing. Kienitz first surveys in almost 100 pages the civilizations of the Near East just before their collapse about 1200 B.C. Then comes a dramatic description of the collapse itself, which gave opportunities for independent roles to the Phoenicians, Philistines (with whom he links the Hebrews), Etruscans, and Sardinians. As Greece and Rome begin to rise, his attention shifts to the various peoples of Asia Minor, Saite Egypt, and Iran in the eastern Mediterranean, and in the west the non-Roman peoples of Italy. Finally we witness the defeat of Hannibal, who tried in vain to save the independence of the Mediterranean world (a view borrowed from E. Meyer).

Such a panorama gives Kienitz room to browse widely over ancient and sometimes modern history, including the early temples of Malta, the appearance of the alphabet, the "Philistines" of recent German history, and anything else that seems interesting. He judges the Etruscans and Phoenicians with special favor but utterly damns the Romans; his picture of the Persians is weakened by a lack of knowledge of the increasingly varied native sources.

Even a book for a general audience should have a clear focus and some precision of argument, neither of which appear in the present work; the shreds of archaeological, linguistic, and literary evidence are not synthesized. Nor can Kienitz always be trusted on the factual level. The western expansion of the

Phoenicians is dated much too early and extended far too widely; the equation of Achaeans and Achchiyava is now being abandoned. The most praiseworthy aspect of the work is its generally flowing style; the notes are few, and the bibliography sadly curtailed and spotty. Perhaps the proper title for the book should be *Schatten ohne Substanz*.

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PETER SALWAY. *Roman Britain*. (Oxford History of England.) Oxford: Clarendon Press of Oxford University Press. 1981. Pp. xx, 824.

This book is a replacement of R. G. Collingwood's work on Roman Britain, itself part of the classic work *Roman Britain and the English Settlements* (1936), the first volume in the Oxford History of England. The task of producing it has plainly been a daunting one. First, Collingwood was a brilliant stylist and the long influence of his book on Romano-British scholarship was not least due to the remarkably persuasive way in which his arguments were presented. Second, the study of Roman Britain has been transformed over the past three decades by a flood tide of discovery in almost all aspects of the subject. When Collingwood was writing his book, the annual summaries of work on Roman Britain made up only a few pages in the *Journal of Roman Studies*. Now they account for a substantial entry in *Britannia*, a journal largely devoted to this one province of the Roman empire. It is, then, not surprising that this is only the second full-length history of Roman Britain to appear since Collingwood, the other being Sheppard Frere's outstanding *Britannia: A History of Roman Britain* (1st ed., 1967; 2d ed., 1974 and 1978). It is fair to stress the difficulties faced by Peter Salway and by any other writer on this complex theme. The available data, particularly from archaeological sources, are now mountainous in bulk and they continue to accumulate at a perplexing rate, as Salway notes in his preface (p. ix). This poses severe problems for someone, like Salway, who has been away from active field work in Britain for more than a decade. This may allow him a certain necessary detachment, but it means also that, despite his evidently wide reading, some of the subtler currents of present thinking have proved elusive.

One of the major problems to be solved by any writer of a history of a Roman province is: How far back into pre-Roman times should he go? And how detailed must the treatment of prehistory be? Salway offers us a short and very general account of "The British Background." He is, rightly, cautious about the thornier matters concerning Belgic immigration into southern Britain, but he provides no

very clear picture of the tribal society that the Romans were later to conquer and rule. There is little new to be said about Julius Caesar's two expeditions to Britain, and the following section, "From Caesar to Claudius," leans rather heavily on C. E. Stevens's famous paper of 1951, which has always needed to be read with care, now more than ever. Much better is the treatment of the Claudian invasion, though there are evident errors in the dating of sites. Usk is a Neronian, not a Claudian, fortress, and there is as yet no evidence that Vespasian's westward thrust established Roman forces as far west as the Exe Valley before A.D. 47. Salway rightly knocks on the head the idea of a linear frontier based on the Fosse Way, but he does not sufficiently stress how fluid the period of campaigning actually was. As evidence accumulates from forts in southern and midland England, it becomes clearer that the Roman advance was not the steady progress several modern commentators picture it to have been. The forty-odd years of campaigning needed to reduce most of Britain to Roman rule, from A.D. 43 to about 85, is the most fully documented period of Romano-British history, and Salway is to be commended on preventing his coverage of these years from dominating the rest of the narrative. Some might complain that much detail has been sacrificed in this section, but the mass of archaeological data is such that the line must be drawn somewhere. By and large, the balance between narrative and description seems well adjusted.

The search for a northern frontier during the second century is clearly described, again within sensibly judged limits. The pace of fresh discoveries, on the Antonine frontier in particular, has obviously posed major difficulties, but the boundary of what we actually *know* is always clearly distinguishable in the account. The third century fares less well, as it usually does, not least because the literary sources are virtually silent from Caracalla to Diocletian. There is, however, much vital archaeological information that is not assessed here, particularly from the countryside. Perhaps the weakest section of the volume, however, is that which deals with the later fourth and early fifth centuries. This is one of the most interesting phases of Romano-British history, and there have been excellent contributions to it lately (though some have appeared too late to help Salway: for example, Charles Thomas's outstanding *Christianity in Roman Britain*). It is often difficult to see where Salway's argument is leading amid the quotation of other writers' theses. Some of these are given a status they do not deserve: for example, J. Wachter on fourth-century urban fortification and J. N. L. Myres on Pelagianism in Britain. Others, notably E. A. Thompson on the critical years 406–410, are not adequately assessed.

A substantial part of the volume, about one-third, is devoted to Britain under Roman rule. This is a good, brief account of Romano-British life and culture that will attract much attention from a wide readership. For the specialist there are too few references to the primary archaeological sources.

It is easier to criticize than to praise so broadly based a book. There is much that is good and enduring here. Probably the truth is that Roman Britain can no longer be confined to one volume. Britain from the first Tetrarchy to the mid-fifth century seems worthy of separate treatment and the same is true of the province in the early empire. In that direction lies the way forward.

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MALCOLM TODD. *Roman Britain, 55 B.C.—A.D. 400: The Province beyond Ocean*. (Fontana History of England.) Atlantic Highlands, N.J.: Humanities Press or Harvester Press, Sussex. 1981. Pp. 285. \$25.00.

R. G. Collingwood remarked in the bibliography to his part of *Roman Britain and the English Settlements* (1936) that "modern books wholly dealing with Roman Britain are not very numerous." Times have changed radically, and Malcolm Todd can justly refer to "a flood of books" on this subject that have appeared since the 1960s. But as he also notes, they have included only one history, *Britannia* (1967), by S. S. Frere. One must now add the massive replacement for Collingwood (in the Oxford History of England) by Peter Salway (1981). The great mass of periodical literature, conference proceedings, and other publications of the series *British Archaeological Reports*, and the annual journal *Britannia* (since 1970 hived off from the *Journal of Roman Studies*), reflecting the great increase in archaeological work in Britain, make the task of synthesis difficult. There are also some notable recent monographs, of which Todd has been able to consult A. L. F. Rivet and C. Smith, *The Place-Names of Roman Britain* (1979). It is a pity that he could not make use of C. Thomas, *Christianity in Roman Britain to A.D. 500* (1980), which ranges widely beyond its immediate subject.

A number of detailed comments may be of assistance. It is good to see Cogidubnus presented as *rex magnus* (p. 72), following J. E. Bogaers in *Britannia* (1979), but a pity that on page 78 D. E. Eicholz is ignored (*Britannia* [1972]) on Vespasian's legionary command; Petronius Turpilianus received his *ornamenta triumphalia* (p. 93), not for his work in Britain, but for unspecified services to Nero after Piso's conspiracy was unmasked; Antoninus Pius is oddly described as "an Italian in the Catonian mould" (p. 149)—his colonial descent (Nemausus) is well attested, his resemblance to either Cato dubi-

ous; regarding "the British legates" (p. 162), P. A. Brunt, in *Classical Quarterly* (1973), showed that Dio's *huparchontes* must just mean *ontes*—that is the soldiers, not legionary *legati*; and on the same page the arithmetic is astray when the 1,500 javelin-men are said to be equivalent to three legions; Severus is said to have had "no other provincial command" than that of Pannonia Superior (p. 166)—yet he had governed Lugdunensis and Sicily; "and about three hundred earthworks" (p. 196) should read "and, about 300, earthworks"; the terminal date of the Gallic empire fluctuates from 273 to 274 (p. 204 n.); the British descent of the third-century usurper, Bonosus (p. 207), deserves a rest after the labors of Ronald Syme and others on the *Historia Augusta*—but the better-attested British parentage of the fourth-century usurper, Magnentius, could usefully have been ventilated; Victricius's visit to Britain (p. 238) can hardly be as late as 400–05, since Ambrose was dead by 397; Pelagius was not a priest (p. 238), only a monk.

Nevertheless, Todd has achieved a balanced, readable, and reliable introduction to the subject, which may be commended safely to students of Roman Britain who require an outline history, as well as to historians of antiquity who wish to check a remote corner of the Roman Empire.

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MANFRED CLAUSS. *Der magister officiorum in der Spätantike (4.–6. Jahrhundert): Das Amt und sein Einfluss auf die kaiserliche Politik*. (Vestigia: Beiträge zur alten Geschichte, number 32.) Munich: C. H. Beck Verlag. 1981. Pp. vii, 252.

Not the least of Constantine's innovations was the establishment of a new department by combining a number of clerical, postal, intelligence, and military agencies. This strange amalgam maintained a vigorous existence for nine hundred years as a major element of late Roman and Byzantine bureaucracies. In 1919 Arthur Boak published a pioneering study of the department, and now Manfred Clauss has drawn on the scholarship of the succeeding sixty years and his own work to produce a new synthesis.

First Clauss reviews the evidence for establishment of the department, narrowing the date to the years A.D. 297–303. Then he discusses in detail the seven major divisions of the department, ranging from the palace secretaries, messengers and torch-bearers to secret agents in every department, province, and headquarters. Chapters 3 and 5 analyze the powers and position of the master himself, concluding that the key to his power was the close relation to the emperor (*Vertrauensstellung*) that he enjoyed. Chapter 6 surveys the history of the de-

partment from Constantine to Justinian. These sections essentially review and correct Boak's work. Chapter 4 breaks new ground in examining the social origins of the masters; Clauss concludes that they were originally of the lower class, and even in the fifth and sixth centuries the department remains "a domain of the climbers." (But the correspondence of Libanius and also of St. Augustine does not, in my judgment, sustain this.) Two prosopographic appendixes follow. One expands, corrects, and replaces Boak's list of masters, the other gives a splendid list of all the known secret service agents.

In effect, then, Clauss has brought Boak up to date, and he has done this task with accuracy, thoroughness, and brevity. What one misses, however, is an advance in analysis, in interpretation. It is, for example, surely of significance that the department continued to be run by men sprung from the lower and middle classes while the prefectures, especially in the West, came under the domination of the landed aristocracy. An even more important matter, never touched upon, is the struggle for power between the native, "Roman" civil service and the "barbarian" armed forces (V. Sirago, *Galla Placidia e la trasformazione politica dell' Occidente* [1961]).

Finally, there is the role of the master as chief intelligence officer, at once director of the empire's FBI and CIA. In the very short conclusion—two and a half pages and a chart—Clauss does suggest, without any argument or discussion of implications, that in fact it was this role that gave the master his great power. But that should have been the starting point of this study, not its conclusion. Still, it would be unfair to conclude without recognizing the solid contribution this work makes to the study of imperial administrative history.

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VIRGIL MIHĂILESCU-BÎRLIBA. *La monnaie romaine chez les daces orientaux*. Translated from Romanian into French by RADU CREȚEANU. (Bibliotheca Historica Romaniae, Monographies, number 23.) Bucharest: Editura Academiei Republicii Socialiste România. 1980. Pp. 312. 19.50 L.

This is a useful book that sketches the use of coins in Romania, beginning in the fourth century B.C. in southern Moldavia. The early coins are Greek, especially Thasian, Macedonian, and "barbarous imitations." By the first century B.C. the region had developed a money economy that was dominated by *denarii* of the Roman Republic. Roman silver remained dominant until the early third century; later

in that century the hoards are mostly of Roman bronze with some gold; in the fourth and fifth centuries, the coins disappear, the area suffers from invasions of the Huns, and there is a return to "l'économie naturelle." In spite of the title, the book is not narrowly limited either in geographic area or subject: there are many comparisons with similar developments in and out of the empire. Literary and archaeological evidence supplements the detailed numismatic material. Commerce, trade, and trade routes are discussed, and there is an extensive bibliography. Thus, the work is valuable for general economic history.

The monograph does have limitations: much is inferred from a small body of evidence. The total number of Roman coins Mihăilescu-Bîrliba is able to catalogue from this region is less than 1,500. He sometimes laboriously analyzes differences in coin finds and periods that are probably insignificant. Still, we have to work with the evidence we have, and the author is judicious. The coin hoards and other finds published here in numerous tables and an extensive appendix have potential as source material for other scholars. For example, some quite interesting implications (beyond Mihăilescu-Bîrliba's own comments) are indicated in the use of the coins of Mark Antony over a long period of time and in an ever-broadening geographic range.

Although Mihăilescu-Bîrliba has consulted numerous works not easily accessible to Western scholars, he seems, conversely, not to have had access to some important Western studies. For example, he makes no use of the now standard *Roman Republican Coinage* by Michael Crawford; indeed, he uses the obsolete chronology of H. A. Grueber for the middle Republic. But since the thrust of his work is on a later period, the consequences are not serious.

One major problem of the period is the disappearance of some coins because of reforms or, paradoxically, the long-continued use of these same coins in regions beyond the frontiers of the empire. We can see this in the debasement and lightening of the coins by Nero and, more than a century later, by Septimius Severus, which led to the melting down of coinage. The result is that many hoards dated to the late Republic or to the Flavian period in Romania (and elsewhere), as Mihăilescu-Bîrliba shows, may actually have been buried at a much later date. It is just not possible to date accurately many such hoards. Mihăilescu-Bîrliba therefore criticizes Romanian scholars who have attempted to derive historical information from such hoards, arguing, for example, that they indicate the areas controlled by the Dacian king Burebista in the middle of the first century B.C. or that they point to troubled periods from Burebista to Decebalus and in the second century A.D.

The maps are almost useless; the reader needs a

good gazeteer at hand. The index, which only lists localities, is inadequate.

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YURI M. KOBISHCHANOV. *Axum*. Edited by JOSEPH W. MICHELS and translated by LORRAINE T. KAPITANOFF. University Park: Pennsylvania State University Press. 1979. Pp. xii, 348. \$18.95.

This book by Yuri M. Kobishchanov is the first scholarly work in English to summarize the present state of knowledge of the important civilization of Axum. Originally published in Russian, which few scholars of the period and region read, it has now been made available to a wider public through the initiative of Joseph W. Michels of Pennsylvania State University.

The new English edition not only has the advantage of being in a language with a wider currency but, more importantly, has also been considerably revised on the basis of new information made available since the original edition in 1966. In addition, another work by Kobishchanov, *Sources on the History of Axum*, has been translated and included, and the editor has written an introduction in which he discusses recent archaeological work, including his own of 1974, and provides a valuable list of Axumite sites known at present and suggestions about the directions in which future research might go.

The main part of the book, that by Kobishchanov himself, starts with a straightforward historical account of Axum from its earliest times until the ninth century A.D., when the kingdom came to an end. This section provides a detailed, accurate, historical narrative based largely on written documents but tends to ignore archaeological evidence, some of which may not have been available to the writer because it has in the main been produced since 1966. The editor's introduction fills some of the gaps in this respect and draws attention particularly to the work of Anfray, which has been important in providing a chronological framework for archaeological material from Axumite times. It is unfortunate that the promising developments in Axumite archaeology, which would have done much to complete our picture of the civilization, have ceased as a result of political conditions in Ethiopia in recent years.

Subsequent chapters dealing with such matters as economic, social, and political organization give a good account of the present state of knowledge and are largely based on written sources. It is likely that when archaeological research can be started once more there will be much to add on these matters.

The author is especially familiar with the epigraphic material and deals with it well in the appendix on historical sources. He could not know that a further Axumite inscription, in Greek, was found at Meroe in 1976, which strengthens the claim of the Axumites that they invaded the Island of Meroe and perhaps attacked the city itself. Two Axumite inscriptions and one coin found at Meroe are the only evidence, other than the Axumite sources, for this event.

The book is much to be welcomed as a valuable addition to the literature on the subject and is necessary reading for all those concerned with the history of Northeast Africa in the first few centuries A.D. and particularly for those like the reviewer, who, working on Meroitic topics, have been unduly ignorant of what was going on in the highlands of Ethiopia. Both the editor and the author are to be congratulated on performing a very useful service to scholarship. There are, however, some errors in the bibliography, and the translation of the names of Russian journals without the transliteration of their original forms will cause trouble to those wishing to find them in libraries.

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MEDIEVAL

GRAY COWAN BOYCE, compiler and editor. *Literature of Medieval History, 1930–1975: A Supplement to Louis John Paetow's A Guide to the Study of Medieval History*. In five volumes. Foreword by PAUL MEYVAERT. Millwood, N.Y.: Kraus International, for the Medieval Academy of America, Cambridge, Mass. 1981. Pp. civ, 556; xiv, 558–1152; xiv, 1154–1726; xiv, 1728–2330; xiv, 2332–2630. \$595.00 the set.

In the preface to the first edition of his *Guide to the Study of Medieval History* (1917), Louis John Paetow observed that since fall 1914 "the stream of historical writing on the middle ages has become thinner and thinner, so that today it is comparatively easy to keep abreast with the literature on the subject. . . ." But the armistice brought a resumption of the scholarly flow, and Paetow began collecting materials for a revised edition. Shortly after his death in December 1928, the fledgling Medieval Academy of America authorized a committee of scholars, chaired by Dana C. Munro of Princeton University, to bring the project to completion. The committee worked swiftly, and the revised Paetow's *Guide* appeared in 1931.

The stream of scholarly publications flowed on, widening into a river, and in 1938 the Medieval Academy resolved to sponsor a third edition. The

director and guiding spirit of the new revision was Gray Cowan Boyce, who, as an assistant professor at Princeton, had served as secretary of the committee that prepared the 1931 edition. The academy planned to sponsor successive revisions of the *Guide* at intervals of approximately fifteen years and set a target date of 1944 or 1945 for the publication of the third edition. But the hope was frustrated by inadequate and delayed funding, by the onset of World War II, by Boyce's move to Northwestern University in 1946, and by the ever-widening river of books and articles. In 1964 the academy abandoned its plan for a comprehensive revision in favor of a supplement—a "Guide to the Literature of Medieval History, 1930–1960," to be organized in accordance with the classification scheme of Paetow's 1917 and 1931 editions. But, in Boyce's words, "money, even in short amounts, was not easily found," and the river flowed on.

Boyce continued his work undaunted. By 1975 he had accumulated such a prodigious number of bibliographical cards as to raise serious doubts about the practicality of ever publishing them. A new academy committee considered the alternatives of reducing the number of entries drastically or of having the cards photographed and reproduced without an index. The committee rejected both these possibilities as violations of sound bibliographical principles. Finally, the academy enlisted the support of Kraus International Publications to typeset and publish the entire supplement along with a computer-generated index. The result is the present five-volume, 2630-page bibliography of medieval historical literature published between ca. 1930 and ca. 1975. "The Medieval Academy of America rejoices," Paul Meyvaert writes in the foreword, "that the supplement which Professor Boyce spent so many decades compiling, sometimes alone, sometimes with the help of a few assistants, can finally appear. . . ." The joy will be shared by all medievalists. Together with the revised and corrected edition of Paetow's 1931 revision, published by Kraus in 1980, the five-volume *Supplement* provides a nearly comprehensive bibliography of medieval history, unique in the English language.

The *Supplement* is both tremendously valuable and seriously flawed. One hesitates to dwell on the imperfections of a project that, without the heroic efforts of Boyce and the commitment of the Medieval Academy, might never have been published. But no responsible reviewer can ignore the central problem—a problem of which Boyce and the Medieval Academy were fully aware: that the organization of the *Supplement* is eccentric and severely outdated. It follows the scheme of headings and subheadings of the 1917 *Guide*, which was itself adapted from the mimeographed syllabi that Paetow had prepared some years earlier for two of his courses at the

University of California, Berkeley: "a general course in medieval history designed especially for juniors, and an advanced course in medieval culture for seniors and graduate students" (preface to the 1917 edition). These course syllabi became the nuclei of parts 2 and 3 of Paetow's *Guide*, "General History of the Middle Ages" and "Medieval Culture" (each divided into two periods: 500–1000 and 1000–1300). To them Paetow added part 1, "General Books," "in order to make the manual as complete and comprehensive as is possible within its limits" (*ibid.*).

Boyce mercifully omitted the lecture outlines that Paetow included at each subheading of parts 2 and 3, and he wisely extended the second time period of each of these parts from 1100–1300 to 1100–1500. But more generally, the parts, chapters, and categories of the 1917 *Guide* persist essentially unchanged in the 1981 *Supplement*. Boyce's organizational strategy was perhaps dictated by compelling practical constraints, yet it diminishes the usefulness of the work. The difficulty is remedied only in part by the computer-generated index, which includes both authors and personal name subjects but does not include short titles. Geoffrey Barraclough's name, for example, is followed by thirty-two undifferentiated page references, R.-H. Bautier's by thirty-eight, and Percy Schramm's by sixty-three.

Both Paetow and Boyce purported to exclude England from their lists on the grounds that excellent bibliographies of medieval English history were readily available (first Gross, now Graves). But, in fact, the *Guide* and *Supplement* weave in and out of English history in unpredictable ways. The Paetow-Boyce organizational scheme provides no place for Anglo-Saxon England, but under the general heading, "The Normans," one encounters such subheadings as "Norman Impact" (including Powicke's *The Thirteenth Century*), "Normandy and England" (including Brooke's *Saxon and Norman Kings*), "Norman Governance of England" (including Jolliffe's *Angewin Kingship*), and "Successors of the Conqueror" (stretching forward to King John). Earlier, within the era 500–1000, one finds under the rubric "Feudalism in England" such surprising entries as Barrow's "The Beginnings of Feudalism in Scotland"; Bean's *The Decline of English Feudalism, 1215–1540*; Jones's "The Rise and Fall of the Manorial System: A Critical Comment"; Pugh's *Imprisonment in Medieval England*; and Ward's "The Coronation Ceremony in Medieval England."

The non-English headings and subheadings tend to be equally informal. The subheading "Medieval Institutions" (p. 87) includes only sixteen entries; among them is Coulborn's *Feudalism in History* but not Cheyette's *Lordship and Community* nor Ganshof's *Feudalism*. On page 81, under "General Surveys," one finds Robert S. Hoyt and John F. Benton's

Europe in the Middle Ages, third edition, 1974 (which does not exist); on page 89, under "Text Books on Medieval History in English," one finds Robert S. Hoyt and Stanley Chodorow's *Europe in the Middle Ages*, third edition, 1976 (which does). The section on "Law in England" (p. 403) includes Thurneysen: *et al.*, *Studies in Early Irish Law* (1936) but omits Richardson and Sayles's *Law and Legislation from Aethelbert to Magna Carta* (1966). The subheading "Normans" (p. 553), under the general heading "The Normans," includes my own textbook, *The Making of England, 55 B.C. to 1399*, and Kendrick's *History of the Vikings*. The subheading "Nobility in England" (p. 1019), under the general heading "Life of the Nobles in the Middle Ages," includes Galbraith's "Literacy of the Medieval English Kings" but not Wightman's *Lacey Family* (1966). Finally, the subheading "The Twelfth Century Renaissance" (p. 1655) contains only five titles. Among them is my own brief anthology, *The Twelfth Century Renaissance* (1969), but not Charles Young's identically titled anthology (1969) or Christopher Brooke's identically titled monograph (1969). All three occur on pages 84–85 under "General Modern Historical Works: The High Middle Ages."

A thorough analysis of the entries under the numerous Paetow-Boyce headings and subheadings will disclose hundreds of similar oddities. In calling attention to a handful of them, my purpose has been neither to supply a list of *errata* nor to belittle the work of the late Gray Boyce, but to make it clear that the *Supplement*, with its outdated and impressionistic organization and its uncommunicative index, will often prove difficult to use. Despite that fact, Boyce's indefatigable labors—as well as Paul Meyvaert's small miracle in arranging for the publication of a comprehensive, indexed Paetow *Supplement*—deserve the gratitude of all medieval historians.

The awesome cost of the five volumes will have the unfortunate effect of placing them beyond the financial reach of all except major research libraries, medievalists of independent means, and reviewers.

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R. H. C. DAVIS and J. M. WALLACE-HADRILL, editors.
The Writing of History in the Middle Ages: Essays Presented to Richard William Southern. New York: Clarendon Press of Oxford University Press. 1981. Pp. xiii, 517. \$55.00.

Like a portrait, a *Festschrift* has a double character: it implicitly portrays the person represented or honored, but it expressly portrays the judgment, skill, and commitment of its makers. The miscellany

presented to Richard William Southern on his seventieth birthday pays tribute to a wide variety of fields adorned by Southern's profound, yet gently humane, learning during the last fifty years. The seventeen contributions range over the epochs between the eighth and the fifteenth centuries. The majority deal with writers of that period or with texts, composed in England and France before the mid-thirteenth century, and a very rich concentration of eight essays considers the writing of history by monastic authors. Nearly half of the contributions discuss historical views of individual writers (Bede, Hincmar of Rheims, William of Poitiers, Anselm of Bec, Honorius Augustodunensis, Romuald of Salerno, Andrew Horn, and John Blackman), and four others explicate texts to show interactions within small groups or classes of writers.

Southern's own study of Western Europe's attitude toward peoples outside its borders is represented by a study of texts describing a Byzantine-Ottoman marriage in the fourteenth century. Three essays (on the historical context of William of Malmesbury's *Miracles of the Virgin*, on confessional writings as sources of historical data in the thirteenth century, and on chivalric literature of the late Middle Ages) testify to Southern's sense of the interplay between historical writing and other endeavors within the vast and fluid wholeness of culture. Only separate reviews of the essays could do justice to the aggregate of specialized learning in this volume, and, undoubtedly, the immediate value of the miscellany will consist in the use that can be made of individual contributions.

Quite naturally, the portrayal of Southern's interests and achievements is only implicit; the work is by other hands. The collection is an express portrayal of historiography as practiced by a self-selected group of scholars, mainly (or wholly?) Southern's students. As such, it and other *Festschriften* will, in the long run, have a value quite different from the immediate one. Disparate as they are, many of the essays betray similar ways of thinking about historical texts that are by no means universal and that are valid within wider contexts of thinking about the historian's task and its function in society. The devotion of labor and resources to the production of a *Festschrift* on historiography is itself a statement of social value, and, in time, studies of *Festschriften*, including prosopographical analyses, may open windows on the institutions and commitments guiding and shaping various schools of historical inquiry.

Fragments of a collective self-portrayal are clearly discernible in this miscellany for Southern. Some of these deal with methods. On the whole, the essays are internalist, positivist, and philological in their methodology. They treat historical works as Leibnizian monads. There is no discussion of the creative interplay between authors and audience, and, in

fact, a number of the contributions deal with writers who appear to have composed their works in isolation and whose texts actually had no audience other than the author himself. There is no discussion of the constraints of genre on particular works. Each text is regarded as a work complete in itself for purposes of analysis, and the objects of inquiry are to establish the stages by which it was composed, the variant readings that it may provide, and the degree to which text and variants may express an author's inner world of knowledge and commitment.

Internalist analysis as applied by a number of the contributors posits individualism. Again and again, medieval writers and their texts are scored for presumed defects—for the limited knowledge and understanding that they display, for bias, for mingling magic and astrology with historical writing, for oscillating across the fuzzy line between fact and romance, for accepting literary figures of speech in the place of historical truth, for presenting events in chronological disorder. Contributors display sympathy for the personal predicaments or aims of medieval authors, but, bound to internalist principles, they do not appraise texts as artifacts of a society deprived of plentiful and accurate printed texts. The shortcomings are sometimes explicitly described as the results of deliberate choice of an author who wished to preach a particular doctrine, to serve the interests of a given institution, or to justify a controversial or dangerous course of action. Elsewhere, the putative defects are implicitly appraised as revealing the motives or personal knowledge of an autonomous author, rather than impersonal constraints of class interest or social faculties and circumstances.

Finally, by the selection of subjects, the collection sets forth an evident commitment to the study of ruling elites. None of the essays considers any persons outside history-making or history-writing classes. Southern himself has written about serfs, but none of the contributions to this *Festschrift* concerns the traces that agricultural or protoindustrial producers left in historical records. Moreover, although some essays deal with theologians (including Anselm, who subsumed history into the timelessness of his doctrine), none considers the roles that heretics and unbelievers played in medieval theologies of history. In fact, one is struck by an absence of attention to destabilizing social and religious elements in this volume, and, indeed, to theories of historical change that undeniably informed some of the works considered. Conversely, one is struck by an attentiveness to elements of persistence, order, and moderation.

There is a second text waiting to be disclosed within the academic studies in this volume, an implicit text with much that is revealing and fascinating to say. This second text, pervading all of the

individual essays, concerns a particularly brilliant band in the spectrum of operating hypotheses about historical inquiry during the late twentieth century.

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KLAUS ARNOLD. *Kind und Gesellschaft in Mittelalter und Renaissance: Beiträge und Texte zur Geschichte der Kindheit*. (Sammlung Zebra, Bücher für die Ausbildung und Weiterbildung der Erzieher, number 2.) Paderborn: Ferdinand Schöningh. 1980. Pp. 201. DM 19.80.

During the brief period in which children have joined their elders as subjects of serious historical investigation, scholars have ventured energetically on the discovery of childhood in the premodern centuries encompassed in Klaus Arnold's useful book. A knowledgeable beneficiary of this lively scholarship, he combines an interpretive essay with an ample anthology of excerpts from contemporary sources, including a number of illustrations, in an ambitious effort to portray "the child" in medieval and Renaissance society.

What emerges from this combination is not, Arnold himself acknowledges, the coherent picture to which he initially aspired. An attempt at synthesis on so broad a scale is probably premature; in this youthful field too many areas and problems remain insufficiently explored. More important, he finds both the evidence and its interpretations too ambivalent to permit the attainment of this larger goal. He has produced instead a substantial introduction to the study of childhood during this long period, offering a remarkably comprehensive guide to the progress of current research.

Drawing on recent studies and pertinent excerpts from the source collection, Arnold focuses in his introductory essay on five essentially self-contained and somewhat disparate topics. Three of these are briefly surveyed: the medieval definition of childhood, the changing image of the child in literature and art, and children's play. More impressive in content and depth of treatment are two longer sections displaying the contradictions that are most striking in the centerpiece of his essay. This is a diptych entitled "The Child and Death," portraying first the social and psychological impact of an omnipresent mortality and then the destructive practices that contributed to it. Similarly, the ambivalence between affection and harsh discipline dominates a final section on "continuity and change" in the treatment of children, with suggested turning points in the thirteenth and fifteenth centuries.

Emphasized throughout his essay, the diversities and deficiencies of our evidence are most fully reflected in the source collection that is this book's

most distinctive feature, occupying more than half of its pages. Arnold presents chronologically and without evaluation seventy-two selections from writings of sixteen centuries, from Tacitus to Thomas Platter; the great majority, however, date after 1200, with many welcome choices from German works, especially personal records, and from humanist writings. Yet, varied as they are, Arnold's texts are predominantly didactic and prescriptive, telling us much about attitudes toward the care and training of children, far less, inevitably, about the realities of their experience. How both attitudes and actualities were affected by changing family structures, relationships, and expectations is a central question often only obliquely illuminated in a book that, for all of its merits, exhibits rather than analyzes the profound ambiguities at the heart of its subject.

MARY MARTIN MCLAUGHLIN
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GALE R. OWEN. *Rites and Religions of the Anglo-Saxons*. Totowa, N.J.: Barnes and Noble or David and Charles, London. 1981. Pp. 216. \$18.50.

The recent stream, if not flood, of Anglo-Saxonica fortunately continues to flow, and Gale R. Owen's work will certainly be useful to beginners, if not to specialists. The chapter titles clearly delineate its scope: "Gods and Legends"; "Everyday Life in Pagan Times," which ranges through idols and temples, sacrifice and festivals, kingship and runes; "Pagan Inhumation and Cremation Rites"; "Pagan Ship Funeral Rites," emphasizing, of course, both *Beowulf* and Sutton Hoo, for which, after an initial "highly likely," the commemorated ruler is confidently referred to throughout as Raedwald; "The Arrival of Christianity," which is the traditional conversion story, with a brief account of early culture, including two sentences on Bede and six on Aldhelm; and "The Viking Age and the Benedictine Reform."

Not surprisingly, this two-hundred-page survey draws on customary materials and interpretations, and it often lapses into textbook summaries, discussing the eighth- and ninth-century Viking attacks in less than five pages (pp. 165–69). More surprising, in spite of a bibliography of about one hundred twenty items, is the lack of especially pertinent scholarship that would have deepened the analysis: E. O. G. Turville-Petre on gods and legends (for example, Balder in England); Kenneth Harrison on dating and the calendar; Vera Evison's major work on grave-goods in the invasion period; Henry Mayr-Harting on the conversion and much else; and others. There are careless imprecisions: there is no "Queen of Denmark" in *Beowulf* nor "Holy Roman

Emperors" in the early ninth century; the kings who paid homage to King Edgar at Chester were not all "of Viking origin"; and so on. Complicated problems (for example, sacral beasts) get short shrift and often only one interpretation. In spite of awkward bibliographical notes in the text itself and a few footnotes, it is often impossible to discover the source for information (unless one knows it). Many and often long quotations (like pp. 98–101 from Ibn Fadlan) interrupt the flow.

On the whole, however, the book is a useful survey of its subject and on some aspects (costumes and jewelry, pottery, runes) particularly helpful. The eighty illustrations and line drawings are well chosen. Too little exists for nonspecialists on Anglo-Saxon paganism and Christianity, and this helps fill the gap by combining many sources in a nontechnical manner. Its virtues make one glad to have this book.

WILLIAM A. CHANEY
Lawrence University

CHRISTOPHER DYER. *Lords and Peasants in a Changing Society: The Estates of the Bishopric of Worcester, 680–1540*. (Past and Present Publications). New York: Cambridge University Press. 1980. Pp. xiv, 427. \$49.50.

Christopher Dyer's book is a detailed and exhaustive analysis of the seigneurial and peasant economy from the tenth century to the Reformation on the estates of the bishop of Worcester, located primarily in Gloucestershire, Worcestershire, and Warwickshire. The book consists of an introduction, describing the sources upon which the study is based, and seventeen chapters, evenly divided between discussions of the bishop's administration and exploitation of his estates and examinations of his peasant tenantry. There are two lengthy appendixes: "Major Officials of the Estate, 1200–1540" and "Profits and Arrears on Individual Manors, 1370–1540." In addition, there are almost one hundred maps, illustrations, figures, and tables scattered throughout the text, as well as a comprehensive index.

The first eight chapters are a model of clear exposition of the complex matter of economic administration. The first chapter primarily describes the early acquisition, settlement, and exploitation of the various properties within the estate up to the late eleventh century. The second chapter is a brief discussion of the impact of feudalism on the estates. The third chapter analyzes the lord's economy from 1086 to 1350, with special attention to the bishop's rents during the period. The fourth chapter—sketchy because of the paucity of sources—examines the peasantry from the eleventh century to the Black Death. The fifth chapter traces the vicissitudes

tudes of the lord's economy in the last period of direct management of the estates (1350–1400), and the sixth chapter explores the bishopric's income from 1375 to 1540. The seventh chapter is a valuable analysis of the real income available to the bishop during the same period. The eighth chapter changes the focus from the lord to the peasantry and contains a detailed examination of the nature of demesne leases, the sizes of parcels, the identification of lessees—of whom the majority were gentry and peasants—and the lessees' exploitation of their properties.

With the ninth chapter, Dyer moves into the world of peasant experience on the estates from the middle of the fourteenth century to the middle of the sixteenth century. It is in this final half of the book that his contribution is particularly outstanding. Drawing upon the surviving economic records of the estates and combining them with material from extant court rolls and wills, he presents a picture of change in peasant society and living conditions that is stimulating and provocative. Beginning with a chapter on population change between 1348 and 1540, Dyer argues convincingly for conscious family planning as a major factor in reduced population, in addition to plague, high infant mortality, and similar, familiar explanations. Subsequent chapters deal with: the tenant population, whose size did not always reflect the changing population; deserted villages; peasant relationships with the lord; changing social distinctions within the rural communities, stimulated by declining rents; the local land market, with important observations on the cyclical nature of families' involvements with tenure; and the peasant holding itself. This last includes an analysis of patterns of peasant agriculture and reinforces the picture of a gradual improvement in peasant living conditions from the fifteenth century. Additional chapters explore the relationship of the peasantry to markets and examine the inner workings of the village community itself. Unfortunately, Dyer's available court rolls are not extensive enough to permit as deep a penetration into this subject as the reader might wish, but his observations are valuable, especially regarding migration patterns and the presence of a dominant elite in the villages.

Dyer's book draws a picture of a multifaceted peasant society, characterized by steady improvement in living conditions and displaying ingenuity and resourcefulness in times of major economic and demographic change. His observations extend our understanding of the nature and experience of medieval English villagers and expand our knowledge of this society beyond the East Midlands. His investigations are based on a careful sifting of surviving documents; they are presented clearly and persuasively and, happily—dare one say it?—with-

out the polemicism that too often mars such work. Equally important, this is a book that suggests issues for continuing investigation—for example, peasant marriage patterns, contraception, and intravillage tensions.

Dyer's book will be of particular interest and value to specialists in medieval rural history, but it will be of equal value to generalists and to early modernists. The final recommendation of this reviewer can only be: Read it, study it closely, then read it again. The time will be well spent.

EDWIN B. DEWINDT
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KATHERINE WALSH. *A Fourteenth-Century Scholar and Primate: Richard FitzRalph in Oxford, Avignon, and Armagh*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xviii, 518. \$65.00.

It is appropriate that the biography of an Irishman whose career took him to Oxford, Avignon, and Lichfield and whose writings were widely circulated in Germanic lands should have been written by a compatriot who has studied in Oxford, Munich, and Rome and who now works in Innsbruck, for Richard FitzRalph's life demands from his biographer a wide range of knowledge and skills. It is the great merit of this immensely learned and meticulous book that every aspect of FitzRalph's career and background receives equally sympathetic and careful attention.

Born in Dundalk into an Anglo-Irish bourgeois family around 1300, FitzRalph studied at Oxford where his *Commentary on the Sentences* attracted attention. He became chancellor of the university and then in 1334 traveled to Avignon, the mecca of all ambitious theologians. The exciting intellectual climate of the curia stimulated FitzRalph to theological activity and consequently advanced his career; visits to Avignon were a recurring feature of his life. During his first stay FitzRalph was consulted in the controversy over the beatific vision and was rewarded with the deanery of Lichfield. In 1337 he returned to Avignon to prosecute lawsuits on behalf of his cathedral, composed the first part of his *Summa de Questionibus Armenorum*, and was subsequently promoted to the archbishopric of Armagh. He came again to the curia in 1349 to argue Armagh's case for holding the primacy of Ireland. He returned to Avignon in 1357 to defend his radical views on the mendicants, and he died there in 1360.

Interspersed with these sojourns was FitzRalph's work as a pastor and administrator first in Lichfield, then in Armagh. We can observe this in an unusually personal way through the sermon diary in which he recorded both the content of his sermons and

where he preached them. Although promoted as a reward for his theological expertise, FitzRalph proved energetic and conscientious both as dean and archbishop; his denunciations of the violence between the two communities in divided Ireland are grimly topical. It was ironic and tragic for the medieval church that even so zealous a pastor and reformer felt compelled to spend long periods pursuing legal claims at the curia. It was, Katherine Walsh suggests, his practical experiences as archbishop of Armagh that turned FitzRalph against the mendicants whom he pursued from 1350 onward with more zeal than logic. His arguments were later developed by Wycliffe in a way that would have horrified FitzRalph.

Walsh's lucid exposition of the mendicant controversy is a fitting climax to a distinguished monograph whose originality is firmly based on an exhaustive study of the sources and a comprehensive knowledge of the secondary literature. In discussing FitzRalph's early years Walsh produces new material and suggests a revised chronology, and her treatment of his writings is notable for its mastery of modern, continental work. If she is unable conclusively to answer all the questions about FitzRalph, her hypotheses—in what will surely become the standard biography—will not be easily challenged.

A. K. MCHARDY
University of Aberdeen

NIGEL SAUL. *Knights and Esquires: The Gloucestershire Gentry in the Fourteenth Century*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1981. Pp. xiii, 316. \$44.00.

Nigel Saul has written an interesting and suggestive book, which has a wider application than simply a focus on Gloucestershire as is implied by the subtitle. His goal is the ambitious one of tracing and explaining the transformation of knightly and subknightly groups into late medieval "gentry," forerunners of that group so heatedly studied by Tudor-Stuart and later English historians. Sometimes he puts quotation marks around the term, more often not. His concern is not whether this group was rising or declining economically; his lengthy final chapter wrestles bravely with this question, but, as he repeatedly points out, the evidence is simply too meager in quantity and inconclusive in quality to prompt any sort of confident conclusions. The focus, therefore, is on the social process of "gentrification" (a term used sparingly and *always* with quotation marks!), which is measured by, and described in, political and institutional terms: contemporary terminology (governmental and heraldic), military service, office holding, patterns of retainership, and clientage.

The information assembled is important, but its impact somewhat weakened or diffused by the book's structure. Saul identifies a group of some fifty families, holding about half of the manors in the county, but with a significant minority holding only one manor and with a separate, important minority holding manors in other shires. Many had military experience in fourteenth-century wars, but only a handful could be termed professional or career soldiers. More than half held, intermittently or consistently, administrative positions such as sheriff or justice of the peace. At least two-thirds were retained by great Gloucestershire families like Berkeley or peers with interests in the southwest or the marches, such as Despenser and Gaunt.

Saul does not always succeed in conveying a clear sense of a group undergoing social coalescence and cohesion. He eschews "group biography" in favor of more thematic and national emphases. General problems are tested and illuminated by Gloucestershire evidence: clientage (still unfortunately dubbed "bastard feudalism"), Commons' concerns for effective local governance, and the like. Saul does as fine a job as I have ever seen in sorting out the vocabulary of gentility, but he is less plausible in asserting a genuine shire community. Local ties, on his evidence, still seem to me more informed by individual lineage and retaining interests than by a group social and political consciousness. The "community of the shire" is a concept not necessarily more helpful or less slippery than the phrase "community of the realm," favored by an older generation of historians—and be it noted, asserted by them for the preceding, thirteenth, century. An independent, locally integrated "community of the shire" of Gloucester gentry is something more frequently asserted (and desired?) than actually proven or shown in specific operation.

The usefulness and importance of this book far outweighs its limitations. It is a major, in ways pioneering, contribution to the history of the lesser nobility. The higher nobility, fewer in number and far more accessible as individuals, have never lacked biographical, group, and case-study attention by historians. That the lesser nobility are now being investigated with the same care and enthusiasm, by scholars as diverse as Rodney Hilton, Sally Harvey, David Carpenter, P. R. Coss, J. R. Maddicott (whose student Saul was), and R. C. Palmer, is a welcome, indeed long-overdue, development. We are witnessing not only the "rise of the gentry," but also the rise of their historians.

MICHAEL ALTSCHUL
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CHARLES PHYTHIAN-ADAMS. *Desolation of a City: Coventry and the Urban Crisis of the Late Middle Ages*. (Past

and Present Publications.) New York: Cambridge University Press. 1979. Pp. xx, 350. \$35.00.

Coventry was the third most important English provincial city in 1377, but it declined very substantially by the sixteenth century. This study is centered on the period 1500–30, with much attention to the later medieval background. The city's population was halved between 1450 and 1550, and the most crucial decade in the decline, the 1520s, threw up some unique surveys that supply much statistical material: famine, epidemics, taxation, and textile slump together removed 20 percent of the inhabitants. Charles Phythian-Adams probes the demographic, social, and general implications of this disaster. A context is provided by a general analysis of the structures of the late medieval city and a sketched thesis of general urban decline in this period.

Here are many good things: an interesting anthropological approach to social history; sensible and striking observations about the role of public ritual, the guilds, servants, women, and the household. Here is a quarry for the current debates on the impact of disease on late medieval society and the fate of the towns. The book is systematically organized and its style is lively and forceful.

Phythian-Adams, however, attempts both too much and too little: he attempts to place Coventry in a general scheme of urban decay, but this theme cannot be convincingly developed in the space allotted to it; and he does not carry out a thoroughgoing study of the city as a whole, for some classes of documents and many topics are left unexamined—in this area one looks at the whole or risks serious error. This may account for my doubts about the central thesis of profound economic collapse, for it is not rooted in a thorough analysis of the precrisis economy. Colorful writing is welcome, especially where statistics are involved, but the author cannot resist the use of extreme language—for example, the “desolation” of the title, surely more appropriate to desertion rather than mere decline. The same lack of balance can appear in his arguments, which run to extremes liable unfairly to undermine the reader's faith in the author's historical objectivity: he writes of “years of seething unrest” (p. 252) but can only produce one plot (with limited local involvement) and one enclosure riot. Too thin a documentary base has to bear too heavy a burden of conclusion, as when a population estimate of 1434, which is in truth only a rough guess, has to support a century of demographic history.

These faults are sad, for both subject and book have much to offer. This monograph will occupy a central place in late medieval urban studies, but the reader must try to filter the material of value from the tendentious framework in which it is set. Big

questions remain unanswered: Was Coventry's decline typical, or was its history distorted by an earlier boom from which it was readjusting? Was the crisis of the 1520s much more than a chapter of “normal” accidents that happened to coincide?

ALAN DYER

University College of North Wales

ANTHONY GOODMAN. *The Wars of the Roses: Military Activity and English Society, 1452–97*. Boston: Routledge and Kegan Paul. 1981. Pp. 294. \$30.00.

The past year has seen the publication of Bertram Wolffe's *Henry VI* and Ralph Griffiths's *The Reign of Henry VI*. Both volumes, especially Griffiths's book, would have enriched the present book had they been available. Anthony Goodman has produced a bifurcated effort to support his thesis that “military activity does not have to be prolonged, wide-scale and devastating to produce significant social and political effects” (p. 7).

The first portion of the book is a narrative of political and military events based largely upon chronicles. The account Goodman presents is sound, and it provides a useful summary, albeit such information is already widely available. Goodman attempts to show that the Wars of the Roses produced tactical innovations that had relevance to European warfare. To support this claim, Goodman argues that the younger generation of commanders who replaced Richard, duke of York, and Edmund, duke of Somerset, favored a much more mobile form of warfare. There may be some truth to this observation, but one must question whether the shift in tactics was based upon circumstance rather than predilection. Warwick, one of the ablest of the new commanders, showed no desire to leave his entrenched position at Coventry in 1471. The importance of professional military men such as Andrew Trollope is correctly noted in this analysis.

Goodman's interest in military affairs does not extend to the sea; indeed, it stops at the English shore. The important role of Calais in the domestic conflict is barely touched upon, and Goodman displays the unfortunate tendency of too many modern Britons to forget that Ireland is a part of the crown's lordship. Goodman, who is no isolationist, does present a good case for the value of foreign chronicles in spite of the confusion of names and details. His defense of Jean Waurin is marked, but the author's enthusiasm is a bit too great once again when he argues for the value of Waurin's account of the battle of Edgecot, a battle Waurin misplaces at Tewkesbury. These sins of enthusiasm should not, however, obscure the utility of Goodman's narrative.

This narrative serves as an introduction to a

topical treatment of military organization and society, which constitutes the second part of the book. The first three chapters deal with military organization. There is a good deal of interesting information, but the coverage is unavoidably spotty. Those who look here for the detailed type of study that H. J. Hewitt provided for the Black Prince's army will look in vain. Chance survival of disparate evidence allows Goodman to raise a great many interesting questions, but he can rarely answer them fully. One wishes the author would attempt to come to grips with the basic problem of numbers of men who served in the campaigns. The conflicting figures of contemporary accounts are set forth in the notes without comment or analysis.

The concluding chapter presents a fascinating argument that the civil strife generated interprovincial antagonisms, which made it increasingly difficult to launch rebellions that would enjoy general support. This argument has great implications for Tudor historians, but again it needs much more flesh on the bones. The questions Goodman has raised point the way for future research and remind us of the truth of K. B. McFaiene's observation that "a low level of public security was not incompatible with a vigorous national life." This is worth a book.

JAMES L. GILLESPIE
Cleveland State University

MICHEL AUBRUN. *L'ancien diocèse de Limoges: Des origines au milieu du XI^e siècle*. (Publication de l'Institut d'Études du Massif Central, number 21.) Clermont-Ferrand: Institut d'Études du Massif Central. 1981. Pp. 468.

Michel Aubrun has given us an ambitious, but very uneven, study of the large diocese of Limoges in central France in the early Middle Ages. A member of the faculty of the University of Clermont-Ferrand, he spent ten years preparing this work that demonstrates his detailed knowledge of the subject and especially of the many local studies published in Limousin journals, which are often difficult for American scholars to find.

The book is in two parts and it contains a fifty-page bibliography (less than a dozen of the items are works in English) and a brief appendix of several documents. Part 2 is concerned with the origin and development of the Limousin parishes and is the more important segment. Aubrun divides his 750 years into three periods: first, a consideration of the large parishes of the Gallo-Roman and Merovingian era from the fifth through the seventh centuries; next, a consideration of the multiplication of rural churches during the Carolingian period from the eighth to the end of the ninth century; and, finally, a

consideration of the years between 900 and 1100 when churches were growing rapidly in number and new parishes were being created in population centers. His approach, in part, is to group the parishes according to their patron saint and to place the saint into the period when his cult was most popular. For example, many churches were dedicated to St. Martin of Tours during the Merovingian period and to St. Peter or St. Martial of Limoges in the Carolingian. He abandons this format for the last period and concentrates, especially through the use of cartulary evidence, on the establishment of new churches and the return of old ones to the bishops or monasteries during the reform movement of the eleventh and twelfth centuries. His documentation is sometimes rich, but one would like to have cited the earliest evidence for the existence of the parish or church rather than, as is all too often the case, simply have it listed in a grouping by saint's name with only its location indicated. In many instances the source material is meager, but he could have offered much more information if the entire work had dealt with the parishes.

The first part, primarily an institutional survey of only 150 pages on the development of the diocese from its foundation (ca. 300) to 1050, serves neither as a useful introduction to the study of the parishes nor as a satisfactory general overview of the church of Limoges. The material requires much more analysis and a more critical approach to the sources. His treatment of the council of Limoges of 1031 is a good example. He cites long passages from the notorious account of this gathering by Adémar of Chabannes without considering adequately the reliability of the evidence. Also contributing to the weakness of part 1 is Aubrun's failure to examine sufficiently the changing nature of lay domination of the church, popular religion, monastic spirituality, or the cultural life of the diocese.

Misprints abound. Material is missing (for example, p. 131, n. 49) or lines repeated (see, p. 41 in the listing for L. Levillain) or words garbled (as on p. 181, n. 56). On page 112, note 60, he quotes a passage from the testament of St. Yrieix and cites the 1843 edition, while neglecting to refer to his own edition, which is the first document in the appendix! Yet with all its deficiencies, the book does offer much significant information. Aubrun has provided a valuable contribution to the history of the foundation and growth of the parishes of the early Middle Ages.

DANIEL F. CALLAHAN
University of Delaware

PENELOPE D. JOHNSON. *Prayer, Patronage, and Power: The Abbey of la Trinité, Vendôme, 1032–1187*. New

York: New York University Press. 1981. Pp. xii, 213. \$17.50.

This is a clearly written and carefully researched book. Its thesis is that the equilibrium between the internal, religious life and the external, material cares of la Trinité was always precarious and that the balance broke down by the twelfth century, when the monastery's spiritual goals were compromised. This thesis is explored in five chapters, with particular emphasis on the external activities of the abbey.

The social, economic, and political milieu in which la Trinité operated is deftly analyzed by Penelope D. Johnson in text, tables, and three useful appendixes, based squarely on the evidence from numerous cartularies. La Trinité was founded in the 1030s by the count and countess of Vendôme. From the first, it was dependent on charitable donors for its survival, and it was responsive to their needs. Johnson explores in detail the changing social class of the donors: at first every social level was involved, but by the end of the eleventh century local knights were the major source of support. She discusses these donors' motives and subsequent relationship with the monastery. Similarly fruitful is her study of recruitment of both lay workers and monks at la Trinité, which covers such topics as class, motives, and age. Recruits, she notes, came primarily from the immediate neighborhood. The monks, in turn, served the local community, much like a "social service institution" (p. 161). They provided beds for travelers, food for the poor, medical help for the sick, temporary home and burial ground for the dying and dead, prayer for sinners, economic stability for the downwardly mobile, and (unlike modern charities) a physical presence symbolizing power and piety for all. In pursuing its goals, la Trinité vied with similar institutions (particularly with the nearby monastery of Marmoutier) and jockeyed for position and privilege with secular powers and ecclesiastical officials. In every instance, Johnson's particulars derive from a scrupulous adherence to the published and manuscript sources.

The internal life of the monastery, with which this external activity presumably was in precarious balance, is given much less attention. The chapter on la Trinité's "inner workings" (p. 36) discusses "personnel, priory structure, and income from lordship and exploitation" but misses the heart of the matter: the life-style, the ideals, and the liturgy of the monks. These topics are later taken up very briefly, in a chapter focused on "la Trinité's contributions to its society," a chapter largely concerned with the artistic and architectural achievements of the monks.

At the beginning of her book, Johnson notes that hitherto two approaches have been taken by monastic historians: some "focus on the religious life,"

others "scrutinize the economic and agricultural activity of abbeys" (p. 1). (This judgment is not entirely fair. Instances of studies that bridge the gap include J. Fechter's *Cluny, Adel und Volk: Studien über das Verhältnis des Klosters zu den Ständen, 910–1156* [1966] and A. Borst's *Mönche am Bodensee, 610–1525* [1978]). Her laudable goal is to consider a monastery holistically, but her own study falls short of this. However, it does go far beyond an economic and agricultural analysis. It discusses with clarity the interconnections between politics, charity, and the economic, familial, and class motives of people in the Vendôme. Its concreteness and precision will make it a welcome addition to the literature on medieval monasticism.

BARBARA H. ROSENWEIN
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JACQUES CHIFFOLEAU. *La compatibilité de l'au-delà: Les hommes, la mort et la religion dans la région d'Avignon à la fin du moyen âge (vers 1320–vers 1480)*. Foreword by JACQUES LE GOFF. (Collection de l'École Française de Rome, number 47.) Rome: École Française de Rome; distributed by Diffusion de Boccard, Paris, or La Bottega d'Erasmus, Turin. 1980. Pp. x, 494.

The substance of this work consists of statistical analyses of about six thousand wills of the fourteenth and fifteenth centuries, chosen out of the forty thousand or so surviving in the region around Avignon. The text, maps, and graphs present presumably solid information about who made wills, how many provided for funeral processions, how many testators wanted to be buried in churches rather than cemeteries, who were the beneficiaries of charitable and pious legacies, how many masses of various kinds were ordered, and so on. In each case the changes over the two centuries are noted. Jacques Chiffolleau's intention, however, is to move from these data to an understanding of how people thought about death and thence to "mental representations" and the "social history of religion." His method has nothing in common with the objectively valid quantifications it is applied to. His explanation of particular changes is usually *ad hoc*, as, for example, when he associates testators' shifts from the mendicants to the secular clergy as objects of legacies with the solution of the religious crisis of the schism and the councils (p. 251). His more general explanations, on the other hand, involve a comprehensive reinterpretation of medieval history that is interesting but also unrelated to the data by any nexus other than possibility. The book is therefore disconcerting, inasmuch as a clean-cut presentation of statistical data is mixed in with a vast amount of irrelevant information and gassy, repetitive conjecture, some of it suggestive but all of it couched in

the pretentious vocabulary of French "social history." The title would be a good example; for the rest I observe that there is something unwholesome about a mentality that leads a French author to use "tester" to mean "to test" (p. 230) in a book about wills.

The scheme of interpretation begins with the rise of towns, movement of people into and between them, and the consequent rupture of traditional solidarities. The town dweller becomes, so to speak, an orphan and also an individual. His death appears to him not as a routine absorption into the corpus of his ancestors but as a problematical journey that he himself must prepare, program, and provide for. Hence, the increasing use of wills, from the end of the thirteenth century on, for the will as such was the act of an individual emancipated from custom and clan. Hence, too, the great concern for the whole process of passage from this life to the next: the funeral became a procession, money was bequeathed to intercessors (the poor, the religious), charities were provided for, and—above all—masses were ordered in great numbers and various forms. The very rich established chantries for perpetual masses on their behalf, but most people put together packages of masses that would run their course in a certain number of years (typically five). The focus of interest shifted from fear of the Last Judgment to concern for a safe passage into purgatory and a shortening of the time to be spent there. The place of the missing ancestors was taken by Mary, as a sort of "great mother," and by the corpus of souls in purgatory. Thus, even while the clergy succeeded in keeping control of death and its circumstances, the individual was able to take care of himself by his narcissistic computations—so much for the funeral, so much for the poor, so many hundreds of masses. Chiffolleau's belief that he has refuted Huizinga's image of a late-medieval autumn is certainly correct; his recourse to the *deus ex machina* of uprooted townsmen may be too simple.

HOWARD KAMINSKY
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IVAN G. MARCUS. *Piety and Society: The Jewish Pietists of Medieval Germany*. (Études sur le Judaïsme Médiéval, number 10). Leiden: E. J. Brill. 1981. Pp. xii. 204. f 78.

The historical study of Jewish mysticism and pietism has been dominated by European- and Israeli-trained scholars. This trend has been due, to a great extent, to the singular place Gershom Scholem and his students have occupied in the field as well as to the formidable paleographic, linguistic, and analytical skills required by the researcher. It is particularly in this context that one should appreciate the ap-

pearance of the well-written and well-conceived book on German-Jewish pietism by an American-trained scholar, Ivan G. Marcus. Not only is Marcus's competence in reading medieval Hebrew esoteric texts well attested by this volume, but he also brings to his Jewish data the beginnings of a new conceptual framework, borrowed from the work of sociologists and anthropologists of religion. Fully sensitive to the abstruseness and ambiguity of his sources and to the limits of categorizing them by abstract theoretical concepts, Marcus succeeds in introducing considerable clarity and perspicuity.

Piety and Society is best read together with the previous work of Israeli scholars, especially that of Joseph Dan. As Marcus accurately states, the book builds on and departs from these earlier studies. Dan masterfully has studied the Pietists as theological writers; Marcus emphasizes their role as social and religious innovators, focusing his study on nonspeculative, pietistic sources and on social and religious questions in their writing.

The results of such inquiry yield positive results. Marcus first analyzes "the shared vision" of the two major writers of German pietism, Judah the Pietist and Eleazar of Worms, and concludes, as does Haym Soloveitchik, that at its core lies a peculiar understanding of the infinite divine will that demands of the Pietist an infinitude of additional safeguards and prohibitions beyond the norms of traditional Jewish practice. This common ideology need not, however, obscure the important differences between the two thinkers. Marcus argues convincingly for the unlikelihood of Eleazar's authorship of any part of the Pietists' major work, *Sefer Hasidim*. On the basis of this assumption, he skillfully reconstructs the evolution of German pietism from the radical, sectarian phase of Judah to the conservative, personalistic mode of Eleazar. With this distinction, he is able to point out the differing goals and strategies of each author's penitential program. In illuminating the fundamental differences between the Pietist authors and placing them in their broader sociological and institutional context, Marcus makes his most significant contribution to the study of German-Jewish pietism as well as to the social history of religious communities in premodern Europe.

Yet Marcus's investigation is not totally satisfying, as he readily admits. Most frustrating is his inability to draw any conclusion regarding the historical causes of German pietism, its genesis in the twelfth century, its notable transformation from Judah to Eleazar, and its eventual demise. Lacking adequate documentation, Marcus is unwilling to attribute pietism to anti-Jewish persecution, to Christian influence, or to the challenge of French-Jewish talmudic scholasticism, as previous scholars have argued. Marcus's healthy skepticism about the historical

reconstructions of others certainly is well taken; but surely he eventually must opt for some causation, no matter how tentative it may be. Without some understanding of causes, his internal analysis remains detached and disembodied from the political and religious ambience of medieval Germany and from the history of Jewish pietism in other ages and societies. In this vein, one might hope that Marcus's further research will entail a more systematic and exhaustive comparison of the idea and praxis of medieval Christian and Jewish pietism, following similar lines drawn by this penetrating analysis.

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MARVIN B. BECKER. *Medieval Italy: Constraints and Creativity*. Bloomington: Indiana University Press. 1981. Pp. ix, 242. \$17.50.

Medieval Italy is an essay in interpretation reflecting two trends currently revising our understanding of the Italian Renaissance: it stresses the continuities between the Middle Ages and Renaissance and it drives yet another coffin-nail into the myth of secularism as a *sine qua non* of Renaissance civic life. This book reflects an equally significant development in Marvin B. Becker's own outlook. As he notes in his introduction, he was inspired to go back to the period from the eleventh to the thirteenth centuries to resolve nagging doubts that remained in his mind after the publication (and, one might add, the critical discussion) of his well-known earlier studies of *trecento* and *quattrocento* Florence. This concern explains the selectivity of the book under review. Its title is really a misnomer. Becker seeks not so much to add to what historians already know about medieval Italy as to extract and synthesize those aspects of the recent scholarship on it that shed light on the genesis of the Italian Renaissance as he now envisions it. He plans to complete the story with a second volume pursuing his new view into the fourteenth and fifteenth centuries.

In the book before us, then, "Italy" is not the whole peninsula but only those parts of central and northern Italy, excepting Venice, where the classic urban life of the Renaissance later flourished. Becker's "Middle Ages" refers only to particular aspects of the medieval experience in those regions that he thinks connect it with the high Renaissance. And, his chief understanding of that connection is one that scraps the commercial revolution of the eleventh and twelfth centuries as a monocausal explanation and replaces it with a view of the same period in which God and profit were linked in an active, if sometimes uneasy, partnership.

The thesis of a conjunction rather than a disjunc-

tion between religion, economic and social developments, and urban institutions is spelled out, with some help from the vocabulary of structural anthropology, in a lengthy initial chapter on lay piety and social change. Becker then elaborates these themes, sometimes rather repetitiously, in two chapters on the eleventh and twelfth centuries and extends them into the thirteenth century in his final chapter. He delineates the take-off mood of the eleventh and twelfth centuries by contrasting it with the attitudes of Italians in the eighth through tenth centuries. He describes the earlier period as an "archaic" culture dominated by a quest for security and a fear of risk, reflected in its religious, social, and economic behavior. The early Middle Ages is typified by its taste for the objective and concrete, for the formulaic and ritualistic, the tribal and the collective, the personal and the tangible. The new mentality that starts to replace it in the eleventh century is marked, on the other hand, by a willingness to take risks and a faith in the durability of human associations entered into by voluntary choice. Its social, economic, and religious behavior reveals a taste for the abstract and subjective, a wish to validate social and spiritual relationships in terms of love, trust, and inner intentionality, and the formation of social and moral identities linking the individual to others with shared values and to the wider community. The psychic venture capital now available is invested in phenomena as diverse as the long-term loan, the commercial partnership, the guild, the lay religious confraternity, the hospital, the almshouse, the voluntary poverty movement, and the new affective approach to Christian spirituality.

Becker's assemblage of secondary literature certainly supports his characterization of these two successive stages of medieval history. However, the descriptive and diachronic structural model he uses to formulate this information makes it difficult to see whether the religious attitudes or the social and economic institutions produced the chief dynamic in each configuration. It also fails to account for the change from the age of insecurity to the age of confidence. On the first point Becker himself is not always clear, arguing the case both ways, and on the second he offers no explanation at all. He does mention the Saracens and Hungarians twice, in passing, but one is left to presume that the cessation of their recurrent rampages in Italy by the late tenth century, and the conquest, pacification, or incorporation of other aggressors by the late eleventh century, is a suggestion too banal to meet the demands of a conceptual apparatus satisfied only by Lévi-Straussian "deep structures."

There are a number of more specific problems in this book as well. Becker cites family identification as a manifestation of the early medieval tribal mentality. At the same time, he notes that, far from

weakening in the upbeat eleventh and twelfth centuries, family ties actually strengthened as the family became more of a legal unit for fiscal and political purposes. Also, far more religious ritualism and concreteness survived into the High Middle Ages and beyond than Becker acknowledges. He would have been helped here by the research of Richard C. Trexler, to which he does not refer. It is hard to think, with Becker, that Otto of Freising had a clear-eyed view of the Italian scene, given his ignorance of the inter- and intraurban factions in twelfth-century Lombardy and his stunningly naive belief that the Lombard towns could be cowed into obedience on the Holy Roman emperor's command. Lanfranc's refutation of Berengar's Eucharistic doctrine was not based on Aristotelian logic, as Becker states, but on the middle Platonist technique of equipollent arguments, as Richard W. Southern, whom he cites, has shown. Anselm's forensic theory of the atonement is sometimes used by Becker as an index of the new religious sentiment of his age and at other times is treated as an example of the "archaic" legalistic approach to theology. Anselm's semantics is described as a break from early medieval objectivism. Yet, the traditional thinkers he is alleged to have departed from did not live between the eighth and tenth centuries but were Augustine and a Boethius whose earliest logical commentaries and translations had just become available in Anselm's century. Indeed, the history of theology and philosophy is definitely not Becker's long suit, as is seen in his attempt to classify Lanfranc and Anselm as representatives of the Italian milieu when they were card-carrying members of the northern French and Anglo-Norman intellectual world.

These difficulties certainly cloud Becker's interpretation. At the same time he deserves warm praise for neatly sidestepping some of the mistakes committed by historians trying to push the origins of the Renaissance back into the Middle Ages. He resists the temptation, in arguing for the rise of individualism and subjectivity in the twelfth century, to ignore the rise of the corporate institutions that were just as typical of that period. As Becker rightly observes, freedom, in the High Middle Ages, was not opposed to law or authority but simply inclined men to replace outworn forms of association with newer institutional embodiments of their new values and attitudes. Another contribution of this book is Becker's well-documented dissociation of the idea of lay culture from the idea of secularism. The progressive growth of lay piety and its increasingly civic-spirited focus on philanthropy, community service, and public salvation in the High Middle Ages suggests the ease with which this deepening appropriation of Christian social ethics by lay people could merge with the classical rationales for civic life and social responsibility articulated by Italians in the

Renaissance. It is with the prospect of that future merger that Becker concludes *Medieval Italy*. In so doing he leaves the reader with a question analogous to the one left by his own earlier work: Given the widespread nature of the religious and social changes that Becker records, within Italy and elsewhere, why did they have different cultural and institutional outcomes in the major Italian city-states of the Renaissance? If an answer to this question is forthcoming, we will have to look for it in the author's projected sequel to this book.

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WILLIAM M. BOWSKY. *A Medieval Italian Commune: Siena under the Nine, 1287-1355*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xxii, 327. \$29.50.

In this excellent work, the culmination of twenty years of study and research, William M. Bowsky contends that the city of Siena during the late thirteenth and early fourteenth century was the most civic-minded of the Italian city-states. The city that produced the Palazzo Comunale, the *Maestà* of Duccio di Buoninsegna and that of Simone Martini, and the frescoes of Ambrogio Lorenzetti on the "Allegory of Good and Bad Government" was the most public and institutionally oriented of the communes in its cultural patronage and achievements. In Siena the style of cultural investment resulted in an urban architecture characterized by palaces of the nobles built in conformity with the style of public building and not by splendid private chapels of great families such as those of the Bardi and the Peruzzi in Florence.

Siena's medieval period of cultural efflorescence coincided with the oligarchical rule of the Nine (1287-1355) and it is in the unusual stability and continuity of that regime's government that Bowsky finds the key to medieval Siena and its cultural achievements. Basing his analysis on archival records, Bowsky meticulously studies the Nine's domestic and foreign policies and the evolution of institutions under that regime. He identifies two elements in particular as crucial to the oligarchy's success. One was its willingness to devote extraordinary time and energy to the business of government. By analysis of membership lists from various governmental institutions, ranging from major councils to relatively minor commissions, Bowsky concludes that members of the Nine dominated governmental posts at nearly all levels. Second, Bowsky believes that stability also depended on the regime's ability to retain the support of the magnates. Although theoretically excluded from government, in reality the magnates served widely; a few were even in the

office of the Nine itself. Magnates in Siena had a greater role within the ruling elite than their counterparts in other cities, and this not only helped maintain the regime but helps explain their willingness to pursue public rather than private patronage in the arts.

Policy making and administrative office holding was largely limited to the "best men," that is, to the *Noveschi* and magnates, but the Nine's policies, although they favored the oligarchy, did not exploit those outside this ruling elite. Consequently, only when faced with starvation did the *popolani* turn to rebellion, and even then the fall of the Nine in 1355 came only under the harsher conditions of the postplague period, when the visit to Italy of Charles IV offered an external opportunity and incentive. All groups within the middling and upper classes thus could feel they either shared in governmental decision making or were served by government. So strong was the oligarchy that it successfully controlled and eclipsed the corporations typical of the Italian city-state, such as church, confraternity, guild, and clan. Under the Nine, Siena was "elitist," not "corporationist," and even the earliest recorded guilds were dominated by "feudal families." Bowsky rightly stresses this characteristic as one distinguishing Siena from its neighbor Florence (and, one should add, from most of the other city-republics), where corporations, particularly guilds, dominated government.

Bowsky describes the structure of *Noveschi* government, the functioning of its institutions, and the social recruitment patterns of its personnel, but in doing so has he explained why Siena was so exceptional? Do we know now why the lesser guilds of Siena failed to provoke conflicts and seek power, a movement so typical in the late thirteenth century in other city republics such as Florence and Perugia? Why did the ruling elite of other cities split into factions, but not the *Noveschi* elite of Siena? As brilliant and thorough as Bowsky's work is, it does not resolve these issues, and, perhaps, given his structural-functional approach and the limitations of the documents, he could not do so. To understand factionalism one needs to study not only the structure of society and the functions of its institutions but also the relationships between people. Perhaps the absence of factional strife, as in the case of Siena, also requires that approach, but as Bowsky informs us, the Siennese sources are largely public, not private, documents and do not lend themselves to such an analysis. The balance of complex relationships that Bowsky himself points to as vital to understanding the longevity and success of the Nine remains necessarily an unexplored hypothesis.

In his beautifully crafted work, full of rich detail but always tightly controlled by elegant analysis and appropriate, well-evidenced generalizations,

Bowsky has constructed a worthy tribute to the city he so deeply admires.

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HENRIK BIRNBAUM. *Lord Novgorod the Great: Essays in the History and Culture of a Medieval City-State*. Volume 1, *The Historical Background*. (UCLA Slavic Studies, number 2.) Columbus, Ohio: Slavica. 1981. Pp. 170. \$10.95.

This new study of Novgorod by Henrik Birnbaum offers a most comprehensive coverage of this medieval Russian republic. In his first volume the author deals with Novgorodian ethnic, political, and social history. The second, which is certainly awaited impatiently by students of Russia, is to be devoted to the artistic, literary, and spiritual past.

Volume 1 begins with a fundamental problem of early Russian history: the role of Novgorod and the Varangians (Vikings of the east) in the foundation of the Russian state. In the oldest Russian historical work—*The Primary Chronicle*—it is mentioned that in 862 the city of Novgorod invited a Varangian chieftain to be its ruler. From him, presumably, the first Russian dynasty issued. For the last two centuries this assumption has been a bone of contention between the Normanist and anti-Normanist schools of Russian historiography. An indirect and cautious answer to this problem is suggested in Birnbaum's book: according to definite archaeological data, the city of Novgorod did not come into being before the beginning of the tenth century. Thus, even if Rurik existed—the author considers that he might have—his role as chieftain of a band of Norsemen that might have settled near Lake Ladoga was presumably of quite modest proportions. The earliest definite sources mentioned Novgorod only around A.D. 950 (Constantine Porphyrogenitus) and A.D. 970 (*The Primary Chronicle* and the *Novgorodian Chronicle*). Birnbaum tentatively says, "There is . . . some truth in the *Chronicle* account of the Varangians' initial penetration into northern Russia." In any case, it was Kiev and not Novgorod that presided over the early destiny of Russia, and until the beginning of the twelfth century Novgorod remained an integral part of the Kievan state.

Novgorod's period of independence came with the division of the Kievan state. Novgorod became a republic in the same fashion as the Italian merchant cities, ruled by aristocratic merchant families. Well before the achievement of its independence, Novgorod became the main marketplace of the northern trade route between Western Europe and Russia. After the Mongol invasion of 1236–40, which did not reach Novgorod, the city won a virtual monopo-

ly in Russian trade with the West. It was a period of blossoming artistic activity, and the Novgorodian churches, icons, and frescoes were the proudest achievement of the Russian arts between the twelfth and fourteenth centuries. The end of its independence—which, as Birnbaum stresses, resulted from feuding among the leading boyar families—came in 1478 when this still economically powerful republic was integrated into the growing Muscovite state.

This first volume of Birnbaum's study is an outstanding scholarly achievement that is most useful for the understanding of medieval Russia. Excellent maps and a vast reference apparatus further enhance the value of the work.

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ELLEN S. HURWITZ. *Prince Andrej Bogoljubskij: The Man and the Myth*. (Studia Historica et Philologica, number 12; Sectio Slavica, number 4.) Florence: Licosia. 1980. Pp. 123.

Ellen S. Hurwitz divides her study of Andrei Bogoliubskii into two parts: the first presents Andrei's efforts to rule Rus and his relationship with the church, the second analyzes the religious and literary tracts that formed the myth of Andrei. Unfortunately the study is marred by unexplained paradoxes and vague terminology that confuse more than clarify Andrei's reign.

To cite but a few of the problems: we are told that Kiev was sacked in 1169 (p. 18) and then in 1168 (p. 46); that the white stone needed to build the churches in Vladimir could be cut off by the Volga Bulgars (p. 15), but was also readily accessible (p. 16). Hurwitz mentions the tribute payments collected from Novgorod and the termination of payments sent to Kiev, but we do not learn what the tribute consisted of (p. 16). She notes that Andrei had the support of the aristocracy (p. 13) and wealthy citizens (p. 14), but then lost their allegiance. Yet nothing is said about who these people are, and the social dynamics of twelfth-century politics are lost in ambiguities. Hurwitz presents an interesting discussion of Andrei's efforts to free Vladimir from the metropolitanate of Kiev, but she finds his growing involvement in the politics and wars over Kiev perplexing (p. 17). Hurwitz takes to task those historians who have minimized Andrei's Kievan roots and have seen him as a break with the past and a precursor of Muscovite autocracy. Nevertheless, she accepts the charge—echoed by many historians—that Andrei sought autocracy (*samovlastie*). But Hurwitz fails to give us an analysis of what *samovlastie* in twelfth-century Russia could be. No conceptual framework of Kievan politics is present-

ed so that Andrei's rule can be judged in its historical context. Andrei is often described as a crusader in his campaign against the Bulgars, but Hurwitz confuses the historical concept of a crusader with a Christian warrior.

Hurwitz's analysis of the myth of Andrei is on the whole convincing. She describes how Andrei hoped to transform Vladimir into a "new Jerusalem" complete with churches, a new Holy Day of the Intercession, the miraculous icon of the Mother of God, and a new saint, Leontius, to rival the saints of Kiev. Andrei himself became a holy prince, defender of the faith, and his death transformed him into a passion-sufferer in the tradition of Boris and Gleb.

Hurwitz's study suffers from a failure to discuss the social and economic dimensions of twelfth-century Russia or the function of the princely myth in other principalities, which would make her analysis of Andrei's reign intelligible. Very little is given to explain Andrei's murder and the social upheavals that followed. The non-Russian reader will not be helped by untranslated terms, and my copy confused pages 1 and 9.

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MARTIN DIMNIK. *Mikhail, Prince of Chernigov and Grand Prince of Kiev, 1224–1246*. (Studies and Texts, number 52.) Toronto: Pontifical Institute of Mediaeval Studies. 1981. Pp. xvi, 199. \$14.00.

Martin Dimnik contends that historians have underestimated the significance of Prince Mikhail Vsevolodovich, who is best remembered for his execution by the Mongols in 1246 and subsequent canonization. The body of this monograph consists of an introduction on the limited historiography and Mikhail's life before 1224, three solid thematic (and hence chronologically partially overlapping) chapters on Mikhail's policies in Novgorod, Kiev, and Galich respectively, a brief chapter on Mikhail's death, a conclusion that recapitulates the detailed summaries at the close of each chapter, and an epilogue tracing the development of Mikhail's cult. All of this material fills 155 pages. The remaining quarter of the book is occupied by four genealogical tables, a glossary, a chronological table of events designed to show the relationship among occurrences in the various regions of Russia (which I found superfluous), four maps, a selected bibliography, and author, source, and general indexes. There are five glossy black-and-white illustrations.

Dimnik set out to write a princely political history on the basis of often conflicting or confused chronicle information, and in this he succeeds. He has obviously mastered the data in the chronicles and the relationships among the chronicles. He has

surmounted the obstacles to a coherent chronology created by the chroniclers' use of three calendars. He does provide a generally credible picture of Mikhail as an influential prince of Chernigov and grand prince of Kiev, who challenged other Riurikids for supremacy in Novgorod and southern Rus in the first half of the thirteenth century. Mikhail's ultimate undoing was not caused by a rival Russian prince but by the machinations of the Mongols, who executed him for political, not religious, reasons.

But by immersing himself so deeply in the chronicles in order to construct his political narrative, Dimnik seems to have assimilated their conception of politics too well. Nests of princes from the same dynastic-regional line, such as Mikhail's Ol'govichi, competed for control of the "golden throne" of Kiev or cities like Novgorod for prestige and the profits of international commerce. Success in the political game depended upon family cohesion and leadership. Luck also helped; it was better if one's enemies were troubled by rebellious nobles, famines, or droughts. This perspective has its merits, but Dimnik makes no attempt to look outside it for relevant matters. Paucity of data would preclude definitive discussion, but Dimnik might at least have posed the question of the economy, society, and political structure of Mikhail's principality to explain his ability to carry out his political stratagems. This is very much political narrative in a contextual vacuum.

Although he is aware of the dangers, Dimnik is not entirely consistent in his use of such suspect sources as Tatishchev or the sixteenth-century Nikon chronicle. But more serious is the problem posed by the silence of the chronicles on motivation. In compensating for this omission he sometimes falls into unpersuasive speculation. For example, his most controversial conclusions derive from the credibility he accords a unique sentence in two mid-fifteenth-century chronicles that Daniil of Galicia-Volynia, among other princes, concluded a peace with the Mongols in 1239. Dimnik utilizes this agreement to explain Daniil's failure to fortify his lands against a possible Mongol invasion and his absence from them when that invasion occurred, among other things. His whole line of reasoning rings false, although the "peace" of 1239 itself requires further study.

Finally, in my opinion Dimnik overplays Mikhail's status as the last and most consistent opponent of Mongol rule, whose cult served as a symbol of resistance to hated foreign oppression. Mikhail did, after all, flee the Mongols in 1239–40, and he did submit to them in 1245–46 rather than risk losing Chernigov. In his vita Mikhail acknowledges that Batu's power as ruler is from God, a Pauline note on which Dimnik does not comment. Although Dimnik

appreciates the difference between history and hagiography (or even history written by a chronicler), he adheres too much to the traditional interpretation of unbridled medieval Russian hostility toward the infidel Tatars.

This soft-covered book is quite well produced, although there are more typographical errors than I would have expected. Dense princely political narrative does not make for light reading, but Dimnik handles it well. Indeed, one welcomes the repetition and summary throughout the text, which do genuinely aid understanding of the complex genealogical and political relationships discussed. I would have preferred a comprehensive bibliography because a number of relevant studies are not mentioned in the notes. These criticisms aside, Dimnik has fulfilled his intended purpose competently, justified putting Mikhail in the limelight, and made a useful contribution to early Russian history.

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DANIEL H. KAISER. *The Growth of the Law in Medieval Russia*. Princeton: Princeton University Press. 1980. Pp. xii, 308. \$25.00.

Although far from being nationalistic, I once pledged not to review any more American books whose authors totally disregard German-language publications on their subject. I am sorry to have to depart from this stand because by now simply too many American scholars seem to be convinced that the study of Russian history is possible without the knowledge of German (or French). In Daniel H. Kaiser's case the dust jacket of his book promises that "he challenges . . . the pioneering Western studies of medieval Russian law," but his bibliography fails to list a single German title on Russian history, not Goetz, Hellmann, Sacke, Jakovliv, Koneczny, or even the pioneering work that was done by German church historians. Oddly enough, G. F. Ewers's book on Old Russian law written in 1816 was used in its Russian translation of 1835, and L. K. Goetz's two volumes on the *Russkaia Pravda* (1910–11) are only mentioned in the disguise of Presniakov's review in the *Zhurnal Ministerstva narodnogo prosveshcheniia* (42 [1912]). This is all the stranger since the author is using a model of legal change extracted from the comparative literature of sociology, anthropology, and law, and his list of studies on African tribal legal institutions is well-nigh complete.

Having started out with criticism, contrary to the usual composition of a review, it is now time to speak about the strengths and positive qualities of Kaiser's book. The author has written an intelligent and readable history of the growth (not of the

"evolution," as he rightly remarks in his preface) of "medieval" Russian law in the period from the origin of the expanded *Russkaia Pravda* (thirteenth century) to the *Sudebnik* of Ivan III (1497). During these three centuries a transition took place from the traditional conception of justice, mainly self-help within a kinship group or a community, to the building up of a judicial apparatus and a power hierarchy for mediation and sanctions. As Kaiser puts it, this was a transition from a horizontal system with dyadic relations between victim and offender to a vertical system with triadic relations for conflict resolution from above. Of course, as everyone working in this period knows, the problem lies in the scarcity of sources. To evade it the author resorts to the comparative research of sociologists and anthropologists (not so much that of Max Weber, but the works of H. Kantorowicz and A. S. Diamond) in order "to find a satisfactory conception of law which embraces divergent social structures" (p. 6). Applying their models to Russia, Kaiser is conscious of the fact that the society did not move evenly; that is, by the end of the fifteenth century there still were many "dyadic" legal relations, but legal development cannot be denied.

After these general remarks on his approach (chap. 1), Kaiser introduces the different sources of law, thus establishing a chronology from the short redaction of the *Russkaia Pravda* to the *Sudebnik* (chap. 2). This survey is very short (only one page plus a genealogy for the *Merilo Pravednoe*), and it necessarily leaves many questions unanswered as far as interpretation and historiography are concerned. (It goes without saying that among the first Western translations of the *Pravda* the German four-volume translation by Goetz [1910–13] is not mentioned.) Chapters 3 through 5 deal with sanctions, the development of judicial personnel, and the patterns of evidence, thus representing the basis of Kaiser's argumentation. On the whole this systematic part of the book is interesting to read, and the author has good points to support his view although, of course, objections are possible in respect to certain detailed problems. The roles of Novgorod and Pskov on the one hand and of the church on the other hand are properly stressed. The latter's part is elaborated on in the final chapter on the "Society and Legal Change," which unfortunately must be called the weakest part of the book. First the reader is informed that legal change was brought about by domestic and external factors. Of the external influences the foreign merchants are mentioned, but not treated (p. 165), and the importance of the church is played down as not decisive. Logically the main impact must have come from within the society. The reader's tension rises high when Kaiser, to prove this, examines a number of social and ethnic terms (apparently without being aware of K. Rahbeck

Schmidt's research on social terminology and C. Goehrke's articles on the *gorod*), but in the end it turns out that it was not the society but the prince who supplanted the customary institutions of justice.

Daniel Kaiser's book, originally a dissertation at the University of Chicago, represents a useful source of information on the facts of the legal change that he is describing. It has a few deficiencies mentioned above (including also the absence of a thorough discussion of the notion of public law). The author would have made his point much clearer by carrying his story up to the time of Ivan IV, who dealt a decisive, if not final, blow to customary law.

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KARL-ERNST LUPPRIAN. *Die Beziehungen der Päpste zu islamischen und mongolischen Herrschern im 13. Jahrhundert anhand ihres Briefwechsels*. (Studi e Testi, number 291.) Vatican City: Biblioteca Apostolica Vaticana. 1981. Pp. 328.

According to the table of contents, this volume is organized into three major parts or segments: "The Evolution and Development of Papal Correspondence with Muslim and Mongol Rulers"; "Tradition and Diplomacy"; and "An Edition of the Correspondence." Despite the author's tripartite division, this book really has only two distinct sections in that the first two parts are prefatory to the last part, which consists of the texts themselves. The first one hundred pages are a general historical survey of papal contact and correspondence with various Muslim and Mongol rulers and kingdoms. The author surveys how, when, and why the papacy became involved with the various Muslim and Mongol rulers. He also gives an overview of the source collections in which the letters have been preserved as well as the formula and internal construction of the letters themselves. Much of this East-West diplomacy dealt with Eastern Christians living in the Middle East, the Crusades, and the early Mongol attacks on both Christian Europe and the Muslim Middle East. Late thirteenth-century attempts to form a political and military alliance between the European Christians and the Mongols of Persia failed, as the overall papal policy focused primarily on missionary and religious concerns rather than a political and military alliance.

After providing the appropriate historical background, the author proceeds in the next two hundred pages to the final section of this volume—the presentation of the Latin texts of sixty-nine letters exchanged between the popes and various Muslim

and Mongol rulers. This segment represents the heart and core of the scholarly contribution of this volume, because it makes these important primary sources readily available to modern scholars. The texts presented are new critical editions derived directly from all of the extant manuscripts and are not merely a new topical collection of previously published texts. Indeed, some of the texts presented here have never been published before—see, for example, numbers 40, 65, and 66. Many of the texts previously published were based on only one manuscript reading and thus were not true critical editions as are all of the texts presented here. The entries are organized chronologically, with the first dating from 1198 and the last from the 1290s. The format of the presentation of each document is noteworthy for its clarity and usefulness. Each numbered entry includes the date, place of writing, and author; a brief historical background and overview of the contents of the letter; the list of extant manuscripts on which the critical edition is based; a list of previously published editions; a list of translations; a list of *regesta* references; and finally the Latin text itself. In terms of completeness the only thing missing is a brief listing of the major secondary studies dealing directly with each of these specific documents, but since the author's primary purpose is to focus on the sources and not the subsequent secondary literature, this may not be a valid criticism or suggestion.

While this volume is an excellent scholarly research tool, it is not without one other minor flaw. The thirteen-page bibliography is especially strong in text editions and source collections and has generally good scope and breadth, but a few pertinent items were, nevertheless, conspicuous by their absence. This reader wonders why the bibliography and appropriate entries did not include texts and reference works like Robert Streit's *Bibliotheca Missionum*, volume 4, *Asiatische Missionsliteratur, 1244–1599* (1928), pages 2–3 for entry number 20 and pages 6–8 for number 21, and so forth; Jean Richard's critical edition of Simon de Saint-Quentin's *Histoire des tartares* (1965), pages 113–15 for entry number 33 and pages 115–17 for number 33a; and Giuseppe Scalia's new critical edition of Salimbene's *Chronica* (1966), pages 298–99 for entry number 32.

Despite the few minor issues mentioned above, this is a well-researched and well-documented study. The introductory historical survey is based on the most recent studies, and the format of each of the sixty-nine entries is clear and readily usable by interested scholars. This volume also contains many very helpful lists and tables—a two-page list of dates, authors, and recipients of all sixty-nine letters; a list of twenty additional texts known through translation and register entries even though the original

letter has been lost or destroyed; a list of incipits and explicits; a list of all manuscripts and original documents used; four tables and charts, giving an analytical overview of the letters and correspondents; eight very useful maps; and a detailed index.

This book is a serious piece of research and scholarship that will be helpful to all students of diplomatic and intercultural contacts between the Christian West and the Muslim and Mongol kingdoms in the thirteenth century. It is an especially worthwhile contribution to medieval East-West relations, as it presents new editions of basic source materials. These new editions of the papal correspondence with the Muslim sultans and Mongol rulers help clarify early European contact with and reaction to foreign cultures and civilizations. This type of early intercultural awareness helped prepare Christian Europe for the later age of exploration and discovery.

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LOWELL CLUCAS. *The Trial of John Italos and the Crisis of Intellectual Values in Byzantium in the Eleventh Century*. (Miscellanea Byzantina Monacensia, number 26.) Munich: Institut für Byzantinistik, Neugriechische Philologie und Byzantinische Kunstgeschichte der Universität München. 1981. Pp. xliii, 266.

The intensification of urban life and the rise of a new class of rich civilians (the *dynatoi* or powerful) after the death of Basil II in 1025 brought havoc to the political fortunes of Byzantium. By the end of the rule of Alexios I Komnenos (1081–1118), who sought to restore the state's pristine glory, Byzantium controlled only two-thirds of the territories it had held a century earlier. By contrast, the fifty or so years of civilian rule were marked by a revived interest in intellectual pursuit, not just as a private hobby but also as a state-supported enterprise. Of the many Byzantine intellectuals of the eleventh century, the most controversial was John Italos, because he was interested in philosophy as such.

Very little is known of the life of Italos, but the succession of trials that were staged in order to eliminate him allow Lowell Clucas to draw some conclusions about the situation of learning in Byzantium in general and to contrast it with that of the West.

Despite the fact that Italos's writings were burned after his condemnation, more than one hundred short treatises have survived and they have been published for the first time only recently. Unlike P. Stephanou and P. Ioannou, however, Clucas is not primarily interested in the works of Italos but in his confession of faith and the records of the trials from

1082. (The anathemas from 1076–77 apparently did not survive.)

With probably one or two exceptions, most of the charges brought against Italos during the trials appear unfounded since his judges, handpicked by the emperor, “were not interested in a rational and fair discussion” but “they were out to get him any way they could, and get him they did” (p. 32). Not since the days of Justinian had a Byzantine intellectual been formally condemned and punished for the content of his teaching. This intolerance of Alexios could be explained by political motives (Italos was from the country of Robert Guiscard) and the emperor’s fear that excessive speculation would be harmful to Orthodoxy. In addition, Clucas enumerates other remote causes, none of which was to be found in the West (chap. 4).

The study concludes with the characterizing of Italos as a philosopher. In contrast to Michael Psellos, mainly an encyclopedist and rhetorician, Italos was a “thinker at the cross-roads between philosophy and theology.” He endeavored to apply syllogistic logic not only to metaphysics and cosmology but also to theology, thus posing a threat to the sovereign authority of dogma (p. 163).

The insights into the organization and place of rational knowledge in both Eastern and Western societies are valuable, although because of space the major points of this comparison suffer from oversimplification. The author could perhaps have more fruitfully concentrated on Italos himself, because what we lack most today with respect to the eleventh century are not surveys but reliable interpretations of the often obscure and scattered sources. Other queries: Is the notion of the unknowability of God as central to Byzantine theology as the author believes? The meaning of “Teacher of the Apostle” should be corrected to “the whole New Testament except the Gospels” and not just “Acts of the Apostles.” The index at the end, mainly of ancient names, is too short and not very helpful since all the footnotes are placed at the end of the book.

These are minor points, and they are meant only to help the author to extrapolate in a future study some of the important ideas dealt with only briefly in the present essay.

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MODERN EUROPE

MARGARET BOWKER. *The Henrician Reformation: The Diocese of Lincoln under John Longland, 1521–1547*. New York: Cambridge University Press. 1981. Pp. xx, 229. \$39.50.

Students of the English Reformation are already familiar with the work of Margaret Bowker from her study of *The Secular Clergy in the Diocese of Lincoln, 1495–1520* (1968). Picking up the story where that work left off, her new book provides a detailed examination of the diocese of Lincoln in a crucial era of change, focusing on the career of Bishop John Longland, Henry VIII’s confessor.

In making her assessment of the changes wrought by the Reformation in the huge diocese of Lincoln, Bowker has drawn on a variety of local sources—the Lincoln Archives Office, the muniments of the dean and chapter, and other provincial repositories—as well as on materials from national archives. The result is an accumulation of data so comprehensive that no scholar is likely to duplicate the effort in the foreseeable future. To facilitate the labors of others, however, the author has made the data base for her statistical compilations available on computer tapes.

From Bowker’s evidence it is clear that John Longland was a “conservative” who was willing to go along with the king on many changes, but who wished to preserve the theology of the old church. In this approach he accurately reflected the feelings of most of the clergy and laity in his diocese. Longland was able to protect many of the old, established institutions from attack by Thomas Cromwell and other Protestant-minded figures by a variety of measures that also protected his own authority within the diocese.

Despite the presence in Lincoln of Lollards, of Oxford University (where Longland was chancellor), and of a seacoast by which dangerous books might be smuggled in, the populace remained as traditional at Longland’s death as when he came to the see. The evidence for this is clearest in the Lincolnshire rising of 1536, which Bowker sees as a protest against too much change too rapidly imposed. The author concludes that Protestantism was not far advanced and not assured of a triumph in Lincoln at the death of Henry VIII. Rather, as Christopher Haigh has shown for Lancashire, there remained a hard fight to convince the parishioners of England that the new doctrines were correct. Significantly, Bowker asserts that it was the successful defensive battle fought by people like Longland that led the English church under Elizabeth I to emerge as a more complex mixture of old and new than the returning Marian exiles would have wished.

One element of the old religion that could not be saved were the monasteries. Longland made particularly strident efforts to improve the religious houses when he first entered the diocese. These attempts were unsuccessful, and the shortcomings of the regular clergy are clearly presented by Bowker. Their failings explain why even a tradition-

ally minded bishop had to give up on monasticism and why the dissolution was carried out with success.

Many other points of interest as to how the bishop managed his finances, the quality of the clergy, and the career pattern of priests might be singled out for mention, for *The Henrician Reformation* is a study that all serious scholars of the period will have to consult. Given the author's use of the term "conservative" to describe Longland, however, it would have been useful for her to have made use of Lacey B. Smith's *Tudor Prelates and Politics*. It is also unfortunate that this otherwise excellently organized and presented monograph should fall victim to the economics of modern publishing and have its copious notes buried at the back. With these minor exceptions, Bowker has emphatically shown that a detailed study of ecclesiastical history, using local materials and modern computer technology, can be a rich source for understanding the extremely complex pattern of events that constituted the English Reformation.

JOEL BERLATSKY
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MICHAEL A. R. GRAVES. *The House of Lords in the Parliaments of Edward VI and Mary I: An Institutional Study*. New York: Cambridge University Press, 1981. Pp. viii, 321. \$39.50.

This is a good book on an important subject. After duly noting historians' neglect of the role of the lords in parliamentary history, Michael A. R. Graves moves to redress the balance in his careful study of the upper House of Parliament in the reigns of Edward VI and Mary I. Lacking the wealth of material that became available in the seventeenth century and later—particularly the main papers of the House, the procedure books, and rolls of standing orders—he makes effective use of the surviving journals, of the original acts at the House of Lords Record Office, and of supplementary material in the state papers and elsewhere. A major portion of the book, which Graves defines as an institutional study, is concerned with the composition of the House, the quality of membership (education and experience), and the relation of the members to each other and, to a lesser extent, to the members of the lower House. These sections are supplemented by three appendixes devoted to membership and attendance. A useful chapter analyzes the importance of the clerks and of assistants to the House. Another discusses rules of procedure and is concerned wholly with legislative procedure. The final chapter evaluates the legislative record.

The House that Graves describes was small, with a relatively low attendance rate. Almost a third of the

peers were newly created, but many had had long political experience elsewhere. The spiritual lords were older and better educated but socially inferior to their lay colleagues. Neither group was cohesive nor subservient to the crown. Graves measures the importance of the House by its effectiveness in legislation, which, in contrast to later periods, was not only its chief but its only business. In Edwardian days, the upper House initiated a substantial percentage of the total number of bills considered and an even higher percentage of those that were ultimately enacted. In 1553–54 the temper of the House changed and it was less successful. Political struggles overflowed from the Privy Council to the floor of the House and destroyed the royal program of bills. Peers deliberately ceased to attend and some took their followers in the House of Commons with them when they left Parliament.

Graves's picture of the House is valuable and his judgment is sound. He picks his way cautiously through controversial questions: the political meaning (or lack of meaning) of absenteeism, the managerial efforts of the crown, the significance of proxies. Occasionally he leans too heavily on John Hooker, who was, as he notes, an Elizabethan and a Commons man. Graves's thesis is clear. The House of Lords must take its place in parliamentary history. It was an efficient, productive body. Its members were on the whole experienced, responsible men. During the Marian period, which was marred by factional strife, disunity at the center shook the House, and its power declined. Whether it recovered in the Elizabethan period has yet to be determined. Nevertheless, it remained a "vital parliamentary force" (p. 202).

Though occasionally repetitive, Graves writes well. His sharp criticism of the work of earlier historians—particularly Pollard, Neale, and Notestein—is unfortunately characteristic of the current polemical tone in parliamentary studies. He comes out firmly on the side of those who see cooperation rather than conflict as the dominant theme in parliamentary history and legislation as the essential business of the institution.

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WALLACE T. MACCAFFREY. *Queen Elizabeth and the Making of Policy, 1572–1588*. Princeton: Princeton University Press, 1981. Pp. 530. Cloth \$40.00, paper \$15.00.

In this sequel to his *The Shaping of the Elizabethan Regime* (1968), Wallace T. MacCaffrey presents a study of selected themes in Elizabethan political history during the years between the St. Bartholo-

mew's Day massacre and the Armada. MacCaffrey divides his new book into three parts. The first section on domestic affairs is devoted exclusively to the problem of religious dissidence, both Protestant and Catholic. The second section, which comprises almost two thirds of the text, deals with England's relations with the continental monarchies and Scotland. The last and briefest section is an analysis of the operation of the Elizabethan political system, focusing on the court and Parliament.

In keeping with its title, MacCaffrey's book tells much about Elizabeth I's role in the making of policy. Here the initiatives often came from privy councillors rather than the queen, but the final determination of policy remained hers. Sometimes Elizabeth made policy by standing still, as was the case in religion, where she apparently regarded the settlement of 1559 as the final act of the Creation. Her refusal to budge on this matter prevented even a moderate change that might have satisfied most reformers; indeed, as MacCaffrey suggests, "without royal obstinacy there might have been no Puritans—no party of critics and, ultimately, of dissenters" (p. 52).

In foreign policy the turning point was the years between 1578 and 1582 when, on account of the threat raised by a resurgence of Spanish power in the Low Countries, the queen abandoned her "traditional policy of cautious neutrality—even of isolationism—in order to launch a new policy of active intervention" (p. 244). Rejecting the idea of openly aiding the Dutch rebels, Elizabeth sought to check Spain via an alliance with France to be cemented by her marriage to Francis, duke of Anjou. MacCaffrey argues convincingly that Elizabeth was sincere in her desire to marry Anjou and suggests that she was motivated mainly by personal rather than policy considerations. Conciliar and popular opposition caused the queen to abandon the idea of marriage, but she continued to make Anjou the basis of her policy concerning the Netherlands, subsidizing his intervention there and inducing the Dutch to accept him as governor. MacCaffrey indicates that the policy failed due to Anjou's "sheer ineptitude" (p. 300), which seems to have been clear to all but Elizabeth. The forthcoming result was what the queen, who could not foresee the events of 1588 and after, above all wanted to avoid: England was forced to go to war with Spain allied only with the battered provinces of Holland and Zeeland.

Enough has been said to suggest the value of MacCaffrey's short-term approach. By attempting to see his period as contemporaries had to, that is, without the "benefit" of hindsight, MacCaffrey has provided a fresh and, I think, convincing picture. The case of Anjou is but a single illustration, albeit a very significant one. As usual in a work by

MacCaffrey, the scholarship is sound and the prose is of high quality.

MORTIMER LEVINE
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ROGER LOCKYER. *Buckingham: The Life and Political Career of George Villiers, First Duke of Buckingham, 1592–1628*. New York: Longman. 1981. Pp. xix, 506. \$35.00.

This is a book that badly needed writing: a careful, detailed, scholarly account of the life and political career of the man who was incontestably the most important person in England, after the king, from around 1620 until his assassination in 1628. Buckingham became King James I's favorite because he was pretty and charming. Roger Lockyer implies, though he does not state flatly, that the relationship was physical, though such an interpretation is difficult to square with the close friendship between Buckingham and the prudish Prince Charles.

At first Buckingham was comparatively unpolitical, interested primarily in feathering his own nest and that of his relatives and secondarily in reform at the admiralty and treasury (since he was patronized initially by the anti-Howard faction at court: these two offices had been headed by Howards and certainly needed reforming). What turned him into an active politician, in Lockyer's view, was his journey to Spain in 1623. Buckingham went reluctantly; it was always risky for a favorite to be away from the source of his fortune for any length of time. His experiences there convinced him that Spain was dealing dishonestly with England and represented a great threat to its safety; on his return he and Prince Charles worked hard and successfully to force the old king to break with Spain. He then plunged into negotiations to create an anti-Habsburg coalition and, once Charles became king, to use force against Spain and, later, against France in defense of the Huguenots of La Rochelle. The bulk of Lockyer's book consists of an almost month-by-month account of Buckingham's involvement in politics, war, and diplomacy from the Spanish visit to his death.

The picture of the duke that emerges from Lockyer's pages is somewhat different from the traditional one, though not as different as Lockyer would wish. Buckingham's most admirable characteristics were his enormous energy and willingness to work, his administrative skills, his attention to detail, his personal bravery, and his dedication to the national interest as he saw it—which was not necessarily the same thing as loyalty to his sovereign. In August 1624 he betrayed James's instructions to his embassy in France to the French ambassador in London, in the interest of promoting a French

marriage for Prince Charles; he feared that if the negotiations faltered, James would revert to his (to Buckingham) dangerous policy of détente with Spain. Far more important than Buckingham's good qualities were his defects. He insisted from the beginning on having a monopoly of patronage at court; Lockyer's denials that he sought this are unconvincing and contradicted by evidence he himself cites (see, for example, pp. 40–41). Buckingham unscrupulously ruined former political allies, such as Cranfield and Williams, when he found it convenient to do so. Worst of all, in Charles's reign he became either deliberately or by default what he had observed Olivares to be to Philip IV, a *privado*, a "political alter ego, the executor of those powers of decision-making which belonged to Philip by birth but which Philip preferred to exercise by deputy" (p. 465). But Buckingham had none of Olivares's talents; he recklessly pursued a line of policy that brought about fighting against France and Spain simultaneously, although he knew—none better—that the English government had no money for such a policy and inadequate armed forces as well. He did not understand the importance of obtaining the cooperation of the political nation in the counties, especially in wartime. His desperate improvisations in a mostly unsuccessful attempt to find men, money, and supplies, on top of his much-resented monopoly of influence and his apparent softness on Catholicism, led to severe political backlash and eventually to his murder. In short, Buckingham was monumentally wrong-headed about politics, and his career was a disaster for the monarchy.

Lockyer is far too kind to his hero. He praises Buckingham as a reformer but is vague about what, precisely, the duke accomplished. He exculpates where he can, and refrains from adverse judgment where he cannot. We nevertheless are much in his debt for pointing out Buckingham's virtues and for giving us this useful and well-written account of the politics of the 1620s from the perspective of a man whom many historians have dismissed as a lightweight. Buckingham was not that; if he had been, it might have been better for King Charles I.

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ANTHONY FLETCHER. *The Outbreak of the English Civil War*. New York: New York University Press; distributed by Columbia University Press, New York. 1981. Pp. xxx, 446. \$39.50.

Anthony Fletcher begins this impressive book by paying tribute to S. R. Gardiner, whose narrative of the first half of the seventeenth century is still the unchallenged basis of all our thinking about the English Civil War. Fletcher modestly (and rightly)

disclaims any attempt to replace Gardiner. His object is rather to draw attention to "aspects of the political process" that have received too little attention (p. viii). For instance, he illustrates throughout the importance of contemporary belief—supported by a good deal of evidence—in the existence of an international popish conspiracy against English liberties and national independence (pp. xxii–xxv, 4–5, 16, 26–27, 61). People then were more aware than many historians now of "the sheer vulnerability of the coasts in face of an armed Catholic Europe" (p. 203).

As befits the author of a good book on Sussex, 1600–60, Fletcher is especially interested in relations between the center and the localities. But he avoids the extremes of some "county community" historians. He knows better than to depict the gentry as ignorant locals, unaware of what was going on at Westminster (pp. xxvi, 374, 380). In their petitions to Parliament, "national ills were seen in national terms" (p. 222; cf. p. 208). Fletcher demonstrates the importance of public opinion and the appeals that both sides made to it by the printing press—a practice going back to the 1620s (pp. xxv–xxvii, 31–32, 75–79, 86–87, 117, 146). Fletcher's analysis of county petitions to Parliament is especially useful: he uses them to trace the growing divisions. He dismisses the traditional canard that these petitions were organized from London, finding that they were rather "an authentic expression of . . . local opinion" (pp. 92–95, 107, 192). In 1640–41 the Laudians were totally discredited (p. 109). In addition to the unpopularity of their doctrine and ceremonies, they had been the only group to put forward a *theory* of absolutism.

In 1641 nineteen counties—mostly in the east but also in the clothing counties of the southwest and Lancashire—petitioned Parliament for the abolition of episcopacy. Only Oxford and Cambridge petitioned in favor of bishops. "It seems probable," Fletcher notes, "that the men who set the national campaign in motion came largely from outside Parliamentary circles altogether" (p. 94). But in the House of Commons the Root and Branch Bill "became more and more blatantly a bid for power by country gentlemen" (p. 104). The episcopate, said Nathaniel Fiennes, was "prejudicial to the honour of the nobility and gentry" (p. 106). The most enthusiastic opponents of bishops were radical sectaries in London, whose iconoclastic activities and subversive talk alarmed many of the gentry (pp. 109–14). From December 1641 "men of lesser social rank" were taking charge of the government of the City (p. 258). The king's flight from London was a triumph for the City populace (pp. 184–85).

In 1642 petitions came from thirty-eight counties (chap. 6). By then the process of collecting armies

was polarizing the nation. Charles was surrounded by a gang of "Cavaliers," whose swords were loose in their scabbards and whose talk fitted ill with the king's "constitutional" pose. Radical sectaries were equally conspicuous in Parliament's army. The main anxiety of the petitioners was to avoid war by finding a middle ground.

When Charles raised his standard at Nottingham, he seemed to be the aggressor: he got very little support from the Midlands and Yorkshire. It was only when he moved to the Welsh marches that volunteers began to come in (pp. 330–33, 356–57). Fletcher rejects "any notion of a simple geographical division into two camps" (p. 368), but something very like it remains (p. 357). "Only three major towns—Chester, Newcastle and Shrewsbury"—were royalist, and they were divided (pp. 394–400; cf. p. 318). Even the petitions of 1642 offer "massive evidence of a new determination to combat and root out popery," with which many associated the Arminian bishops (pp. 200–06, 210–27).

The present reviewer finds this valuable book least satisfactory on the subject of John Pym. Fletcher emphasizes Pym's leadership of the Commons and rightly notes his statesmanlike concern for financial reform (pp. 29, 39). But in his conclusion, contrary to his own evidence, Fletcher speaks of Pym "imposing his own fears of popery . . . on a susceptible population." "The Grand Remonstrance . . . finally did the trick" of making the provinces "grasp Pym's message," "Pym's papist conspiracy" (pp. 409–10, 396). This seems to me out of focus. With hindsight it is easy to speak of "parrot cries," of "the delusion of a Catholic and malignant conspiracy" (pp. 410, 279). But contemporaries were still obsessed by the possibility of a Catholic victory in the Thirty Years' War—a danger highlighted by the successful revolt of Catholic Ireland. What was Henrietta Maria doing on the Continent in 1642? Why was she pawning the crown jewels? We know she was not to return with a Catholic army, but in the mid-forties the Irish rebel army was commanded by a papal nuncio. Pym no more created fear of popish plots than Churchill created the Dunkirk spirit in World War II. Each man succeeded because he expressed the national mood. Fletcher himself shows, contrary to recent suggestions, that there were many papist officers in Charles I's armies (pp. 329, 332–33).

Fletcher adopts the view, fashionable a few years ago, that the Civil War was an accident, with no long-term causes. There was indeed not much initial enthusiasm for either king or Parliament among the gentry of most counties (pp. 326–29). The City's money kept Parliament going; volunteers from London and the Home Counties gave them an army (pp. 23, 338, 342, 346, 355). What Fletcher fails to consider sufficiently is the gentry's awareness of the

danger of a social explosion if authority broke down. They supported whichever side seemed most likely to preserve gentry authority in their area. This was persuasively argued by Brian Manning, and an awareness that the gentry were not the only inhabitants of their counties has distinguished the best recent county research—that of Clive Holmes, David Underdown, and Ann Hughes. Fletcher, indeed supplies evidence to support the thesis (pp. 80, 300–01, 376–78, 382–83, 388, 390, 402). In Staffordshire "localism in its most formidable guise," with full gentry support, collapsed "because a rising of the Moorlanders . . . made it advisable to accept royalist help in the cause of local security" (p. 386). The gentry in fact needed outside assistance to suppress the unpropertied members of their "county community."

But ideological flourishes can be ignored: the main body of this big book is well researched, well organized, and well written.

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MICHAEL MACDONALD. *Mystical Bedlam: Madness, Anxiety, and Healing in Seventeenth-Century England*. (Cambridge Monographs on the History of Medicine.) New York: Cambridge University Press. 1981. Pp. xvi, 323. \$39.95.

Richard Napier, rector of Great Linford, Bucks., was also an erudite and meticulous physician who carefully recorded his consultations over thirty-seven years until his death in 1634. Among his patients were 2,000 mentally disturbed people, and this probably unique source provides the basis for Michael MacDonald's book. Napier's notes make possible a quantitative social analysis of mental sickness. His patients were predominantly female (the sex ratio is close to the modern figure) and surprisingly young, a third of them being in their twenties. Children and old people rarely appeared; MacDonald argues, like Keith Thomas, that both were seen as not fully "reasonable," so that medical treatment was inappropriate. Geographically the patients came from 60 percent of villages in the area: small communities provided no guarantee of mental security.

Clients and their families offered their own diagnoses of the roots of mental disorders; most were seen to spring from stresses within the family: marital discord, the death of child or spouse, lovers' quarrels, and unrequited passion. Part of MacDonald's aim is to use Napier's jottings as a source for contemporary notions of normality. His major finding—the strength of emotional bonds within the nuclear family—is additional and important evidence against the school of Lawrence Stone and

Philippe Ariès. Napier's clients, from almost all levels of society, accepted the ideals of romantic love, companionate marriage, and deep ties of affection to parents, partner, and children.

Only a small proportion of Napier's patients were seen as mad or insane. Most were classified as melancholy (a fashionable condition) or mopish. Religious despair and delusions were also frequent, exacerbated by hellfire preaching. Puritans claimed that anxiety was the fruit of sin and that the remedy was conversion. Napier disagreed and saw mental disorder as the result of humoral imbalance, to be remedied (with the aid of astrological calculations) by traditional medical treatment. But insanity might also be caused by the Devil, demons, or witches. Napier, though cautious, was willing to diagnose witchcraft when he found the evidence compelling and sometimes used exorcism and magical charms. Like many contemporaries he was thus eclectic in both the diagnosis and treatment of mental disorders. After the civil wars, MacDonald argues, this eclecticism disappeared. Religious enthusiasm was labeled a form of madness by the frightened upper classes, and religious and magical healing were frowned upon. A secular psychological medicine triumphed in the later seventeenth century, not because it was any more effective but because it matched the current attitudes of the educated. The result was the development of asylums and disaster for their inmates.

MacDonald's use of the terms "Dissenter," "Non-conformist," and "Anglican" is sometimes questionable, and a few minor lapses have been missed (p. 264, n. 5; p. 296, n. 235). But this is a carefully researched and argued book. Aware of the strengths and limitations of his source, MacDonald has made a valuable contribution to the study of insanity and to wider social and intellectual problems.

BERNARD CAPP
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RUTH K. MCCLURE. *Coram's Children: The London Foundling Hospital in the Eighteenth Century*. New Haven: Yale University Press. 1981. Pp. xiii, 321. \$27.50.

Several generations ago, a history of the London Foundling Hospital would have been expected to contain sentimental vignettes of the founder, detailed description of the physical and moral condition of the inmates, and full accounts of the policies and finances of the governors. In the current generation one might expect the application of the hypotheses and methods of modern family and social historians. Ruth K. McClure provides us with a study that leans heavily toward the traditional approach.

Captain Thomas Coram had a profitable career in trade, and in his later life he devoted many years' effort to circulating petitions for a royal charter to establish a home for foundlings. In its early years (1739–56) the hospital was an important addition to the growing number of London institutions for the sick poor. In 1756 Parliament adopted the idea of general admission of foundlings with public financial support. After four years and about £549,000 this premature welfare project was terminated, and the hospital returned to limited admission and private funds.

The normal operation was itself of some interest. The selection of infants, their distribution to "country nurses," and their return (at age 3 to 6) to the hospital for the period until apprenticeship form an unusual social experiment. McClure has detailed the diet, clothing, education, and housekeeping in exhaustive fashion. She has also mined the mass of corporate evidence to present a large dossier on the governors and their employees. Yet this wealth of detail contributes to the book's greatest weakness. There is not enough analytical structure to sustain the weight of evidence and enable the book to become more than a traditional institutional biography.

One of McClure's contentions is that Coram's foundation was a pioneer effort to deal with abandoned children. The parish poorhouses are dismissed as "filthy, disorderly habitations . . . where pandemonium and vice ruled jointly. In such surroundings the foundling could expect only the care some feeble crone might choose to give it for the short period that it usually survived" (p. 13). McClure similarly dismisses the parish nurse and sets the stage for Coram's enterprise. One hesitates to defend the poor law or its agents, yet the condemnation of reformers has too long and too casually been accepted by historians. Parish care was not utterly hopeless, and probably many of the nurses to whom the hospital sent foundlings were in fact "parish nurses." That the hospital also provided inspectors was the major breakthrough.

McClure claims that Coram's foundation was an original form of "eleemosynary corporation," admitting that at the time "no one realized it" (p. 32). It is probably true that Coram and others were caught up in the joint-stock fever of the early eighteenth century, but once again it is too simple to see the form of this charity as "the prototype for thousands of successors in England and the United States" (p. viii). She terms it an "incorporated secular associational charity," which might have amused Coram had he heard it. Trusts generally were corporations in the eyes of the law, the secular feature was not new to trusts, and association was of course a popular current development (for example, with the charity schools).

McClure has written a good account of the eighteenth-century career of the London Foundling Hospital, but it is disappointing in its attempts to relate the institution to its social context.

R. S. TOMPSON
University of Utah

W. S. LEWIS and JOHN RIELY, editors. *The Yale Edition of Horace Walpole's Correspondence*. Volumes 40–42, *Horace Walpole's Miscellaneous Correspondence*. Assisted by EDWINE M. MARTZ and RUTH K. MCCLURE. New Haven: Yale University Press. 1980. Pp. lxxi, 389; viii, 470; viii, 506.

With the appearance of these three volumes of letters (and with the exception of some forthcoming index volumes), the monumental *Yale Edition of Horace Walpole's Correspondence* finally concludes. Conceived and begun in the 1930s, the product of the benevolent obsession of the late Wilmarth Lewis, the project has delighted and astonished three generations of scholars. Its scope is prodigious, for Lewis proposed nothing less than the publication of every single letter, whether penned or received by Walpole, that the editor's energetic searches, sleuth-like instincts, and ample wallet could bring to light. He proposed, moreover, to lay a foundation of such superb scholarship beneath this house of correspondence that the inquisitive reader could neither miss a now-dark allusion nor misconstrue an elliptical ambiguity. The achievement realizes the dream: the *Yale Edition* stands peerless among the various editorial projects of our day.

The most recent volumes are necessarily anticlimactic. Though the editor asserts that these letters do not represent "the sweepings of the ballroom floor" (vol. 40, p. xxiii), they nevertheless constitute a kind of miscellany. The great sustained correspondences—those with William Mason and George Montagu and, above all, with Horace Mann—have already appeared. Here instead we have Walpole's briefest correspondences: over eight hundred and fifty letters, to or from more than three hundred individuals. Some are famous—Boswell, Burke, Garrick, Gibbon, Hume, Voltaire—but the chief (and compelling) rationale for the publication of the letters is to fulfill Lewis's goal of comprehensiveness.

The Horace Walpole who emerges from these forty-two handsome volumes, though more humane and sexually complex, is rather an amplification than a repudiation of Macaulay's view. Walpole took his delights in social intercourse. He loved gossip, and he attended parties with his ear cocked for the apt *mot*, his eye alert to the revelatory mannerism, and his memory toting up the names of those in attendance. He had, moreover, the true letter-writer's desire to please others. He spoke to

the varied tastes and interests of his correspondents, disclosing his judgments on art and books, teasing epigrams out of anomalous situations, sniggering at the latest lewd remark to be making the rounds, and—on occasions of high drama or sadness—limning memorable verbal pictures. He was most consistently the master craftsman in the 1760s, when he wrote with a controlled buoyancy that surpasses in tautness the more diffuse narratives of his earlier epistolary years and in cheerfulness the somber reflections of his final years. But he is always Horace Walpole, and if it would be hyperbolic to say that he never drafted a dull letter, it would not be an exaggeration to claim that no more witty and entertaining body of correspondence exists in the English language.

Lewis's project has undergone changes since its inception, changes that may be responses to the few criticisms that the scholarly community has mustered over the years. The footnotes have become fuller and more numerous, visibly shortening the ratio of text to supportive scholarship. And even as they have become factually richer, the notes have also increasingly provided extensive references to sources, becoming thereby not merely instruments for instruction but also guides to contextual and interpretive background. These changes enhance the scholarly value of the later volumes and give to the *Yale Edition* a bibliographic and even encyclopedic utility that its originator had perhaps not envisioned.

More important than the changes in the project, however, have been the unforeseen changes in the scholarly environment into which the volumes have been issuing forth. Even as the first volume appeared in 1937 the examination of eighteenth-century politics was undergoing a dramatic change: scholars were learning that division lists and various unpublished collections gave a more accurate insight into the mechanisms of politics than the studiously artless writings of men such as Walpole. John Brooke is currently preparing a scholarly edition of Walpole's memoirs, and when it appears it will perhaps rehabilitate Walpole as a valuable source of political information. But in the meantime a distrust of belletristic sources prevails. And if this shift in the evidential foundations of political history has been reducing the scholarly importance of Walpole's letters for four decades, the more recent de-emphasis of political history in *all* its manifestations and the concomitant rise to prominence of a social history grounded in demography have threatened to make the letters almost irrelevant to the concerns of many historians. As recently as 1968 Charles Mullett could claim that the correspondence carried the reader "into the center of the eighteenth century" (*AHR*, 73 [1968]: 1528). Few would now subscribe to that bald assertion.

But scholarly climates change, and the day of a renewed interest in political history will surely dawn. Moreover, Walpole was far more than merely a commentator on politics. He regarded the whole world of the privileged, the educated, and the powerful as his subject. He toadied to the titled and hobnobbed with the scholarly. He remains the slightly malicious observer of the mores of an entire class; his correspondence provides a route of access to that class. Thus, whatever their particular interests, scholars will continue to turn to these volumes.

What they will find in them will be editing of the highest sort. Three broad and informing principles were chosen early and have passed the exacting test of experience. First—the cardinal principle—the editors decided that the texts should not be tampered with. For the Walpole correspondence the application of this principle has meant chiefly the liberation of sexual and scatological allusions. But prudery is not the only guise the censorial spirit can assume, and thus the example that this edition sets of an undeviating fidelity to the whole of the text is of vital and wide significance to scholarship. Second, the editors chose to organize the letters by correspondents rather than by straightforward chronology. This choice makes particular sense for Horace Walpole since, as Lewis has made very clear, he selected his correspondents for their interests in particular and diverse fields—Mann for politics, Cole for antiquarianism, Lady Ossory for society—and he wanted his correspondence with them to be an ongoing record of his pronouncements about the fields he had chosen them for. But even without this ancillary reason the simple utility of seeing the development of a discussion between two letter-writers makes this mode of organization preferable to alternatives. Third, the editors famously decided to illuminate the texts with an unprecedented array of pertinent facts: identifications of virtually everyone mentioned, information on even the obscurest of events, translations of foreign phrases, monetary conversions, and pertinent quotations from newspapers and diplomatic dispatches and contemporaneous works of scholarship. (The introduction to the most recent volumes singles out the late George Lam, ferret extraordinaire, as the spirit behind the celebrated Walpole footnote; it would be but just if the tireless Lam, through this work, now had earned for himself a footnote in future histories of American scholarship.) The faithful application of these three principles (and of their corollaries) has given an entirely new meaning to our notion of scholarly editing. It is the application of these principles that makes the *Yale Edition* at once magisterial and exemplary.

We may never see its like again. The cost of such enterprises is staggering, sustainable only by large institutions or individuals of extensive means. More-

over, computer technology, and especially the promise of computer networks, may make the production of the multivolume documentary series an unattractive and inefficient use of money, time, and energy. In light of these considerations it is not unlikely that this harbinger of the new documentary scholarship may also be among the last of the great ensembles of documents to be produced between hard covers.

But these letters cannot be viewed simply as an amorphous set of discrete documents: they possess a unity that points to an entirely different dimension of their importance. A number of reviewers have suggested that we now know more about Horace Walpole than about any other person who has ever lived. If the remark applies to his activities, it may be true, though I am not one to dismiss cavalierly the ability of the tenacious journalist of our day to uncover even the pettiest facts about a celebrity. But if the remark applies to Walpole's character, then we must be more cautious. Sheer quantity of output, even when conjoined with a pose of candor, does not guarantee veracity. Even if we can successfully cut through the stylistic conventions that governed eighteenth-century letter writing and that obscure our vision of Horace Walpole, we must never forget the central fact about the man: he wrote in the belief that the eyes of posterity peered over his shoulder. These letters are not the unveiling of inner thoughts to trusted friends, still less the self-revelations of a private diarist determined to consign his jottings to the flames. They are Horace Walpole's creating of Horace Walpole. That is the reason that our view of the man, although richer than Macaulay's, is consonant with his. The strings of consistency that bind the correspondence together across the decades—despite changes in technique, the initiation or end of friendships, and the diversity of subject matters—reveal the essentially artistic nature of the impulse that moved the writer. They mark the newly published letters no less than the familiar ones. It is as a work of art that the Walpole correspondence, cumulatively considered, will best bear the test of time. And it was the pertinacious devotion of Wilmarth Lewis that provided us the opportunity for such a cumulative consideration.

REED BROWNING
Kenyon College

J. A. HOULDING. *Fit for Service: The Training of the British Army, 1715–1795*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xxi, 459. \$69.00.

There are many historical volumes whose titles suggest more than the book actually delivers. J. A. Houlding in pleasant contrast has selected a title

that is modest, for his book offers more than a small study of British army training practices in the eighteenth century. Rather it is an attempt to answer a more profound question: What were the social, political, and economic realities that prevented the British army from engaging in the kinds of exercises in peacetime that would make it a more effective fighting force when war broke out? Houlding's answer to this question is comprehensive and compelling. By examining dispersal patterns in the army, he shows how, at home and abroad, too few occasions where larger units trained together meant that training was reduced to small unit operations—essentially basic training repeated endlessly. The dispersal of the army was necessary because of the size of the British empire and the myriad tasks that small military units performed in the civil sphere. Squads and companies literally marched all over England and Ireland in pursuit of smugglers, heading off industrial unrest, and, in general, acting as a national police force. The army was the police and the police was the army, and this dual function raised havoc with the essential task of training the armed forces to fight abroad. It was only when war broke out that the military function took precedence.

Houlding also notes how inferior training was intimately linked to such problems as manpower. Regiments were perennially short of men, and in most cases the percentage of new recruits was very high. Attrition meant that the officers were always engaged in basic training and the advanced exercises so necessary for battlefield conditions were seriously neglected. If we add to this picture the scarcity of weapons, ball ammunition for target practice, and suitable training grounds for large-scale maneuvers, it becomes easier to understand why the army did not on occasion perform acceptably at war's outbreak.

In short, this is a fine and sensitive book that treats the army not as a host unto itself but as an institution whose success or failure is intimately related to the civilian milieu in which it operates. Accounts of the training of other eighteenth-century European armies would be fascinating for they would allow us to understand with far greater precision how the determinants of training affected the battles of the day and their outcomes.

ARTHUR N. GILBERT
University of Denver

OLIVER MACDONAGH. *The Inspector General: Sir Jeremiah Fitzpatrick and the Politics of Social Reform, 1783–1802*. London: Croom Helm; distributed by Biblio Distribution Center, Totowa, N.J. 1981. Pp. 344. \$42.00.

Oliver MacDonagh has written an interesting book about the late eighteenth-century antecedents of nineteenth-century administrative reform. His subject, Sir Jeremiah Fitzpatrick, is a somewhat shadowy figure. He practiced as a physician in Dublin, but there is no record of his medical training. He was knighted in 1782, but the reason for his advancement is unknown. MacDonagh's purpose is two-fold: to make some comparisons between eighteenth- and nineteenth-century ideas of social regulation and to throw light on the interactions between English and Irish administration, since Fitzpatrick's administrative career from 1783 to 1802 was equally divided between the two countries.

Fitzpatrick became involved in the early 1780s in the active movement of that time for the reform of Irish prisons. He may have come into this work as a physician under the Act of 1778, which required a medical man to be appointed to each prison. In 1784 he published a pamphlet on prison abuses that owed much to John Howard, though he took a more medical and scientific point of view. In 1786 he was appointed inspector general of prisons and worked actively to improve prison conditions throughout Ireland. Like all administrative reformers his sphere of interest steadily grew. He interested himself in private madhouses. He reported on the abuses of the Protestant charter schools. He wrote on penitentiaries for the reform of younger criminals.

By 1790 the tide of prison reform was slowing down. In 1793 Fitzpatrick, who threw himself into any problem that presented itself, tackled an outbreak of fever on army transports in Cork harbor. The health questions thus thrown up led him to England at the beginning of the war with France. In September 1794, he was appointed inspector of health for the land forces, a post that he held until 1802, when he was retired on half pay. Fitzpatrick attracted the support of Henry Dundas, secretary of war, but he found it difficult to achieve very much as he struggled through the complexities of army administration and earned the bitter opposition of the Army Medical Board to his attempts to improve the care of the sick. He had some successes, notably in improving conditions on convict ships, but it seems that the politicians regarded him as a troublemaker and were glad to get rid of him when the short-lived peace came in 1802.

In a final chapter, MacDonagh considers Fitzpatrick's career in relation to his own thesis about the nineteenth-century revolution in government. He finds many resemblances, but it seems difficult to this reviewer to fit Fitzpatrick into any continuing pattern of administrative change. Much of his work centers around the ideas of regulation and inspection that were so central to nineteenth-century

reformers, but the conditions of his time were very different from theirs. It is a very unfortunate error that this book has been incorrectly bound. Sixteen pages have been omitted and a section of another book inserted in their place.

JOHN ROACH
University of Sheffield

DONALD A. MACKENZIE. *Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge*. Edinburgh: Edinburgh University Press; distributed by Columbia University Press, New York. 1981. Pp. viii, 306. \$25.00.

It is perhaps the most pleasing aspect of this book that its content will be of interest to social historians, historians of science, philosophers, biologists, sociologists, and statisticians. It is not the least of Donald A. Mackenzie's achievements that all of these will be able to follow all (or nearly all) of it.

The core of Mackenzie's thesis comes, quite properly, at the very end. He sees science as a process of invention, not discovery, which is "goal-oriented." These goals may all be covered by some such umbrella concept as the desire to predict and control the world, but what is of crucial importance is how the concept is particularized. Mackenzie argues that the process of particularization has to be understood within a social context. This context has two facets: the social interaction of scientists themselves and the development of scientific theories and knowledge that reflect social aspirations, interests, and ideals.

This thesis is articulated through a study of the connection between the growth of modern statistical methods and eugenic beliefs in Britain in the late nineteenth and early twentieth centuries. Three figures—Francis Galton, Karl Pearson, and R. A. Fisher—stand out in this process. All followed eugenic theories, though with Fisher the connection was substantially weaker than with the other two.

The observation of this connection is not in itself new. What is new is an analysis of the connection in a form that makes it of more interest and relevance to social historians. Mackenzie sees Galton's eugenic beliefs as colored by his social position within what N. G. Annan, a quarter of a century ago, called the intellectual aristocracy. Galton's eugenic utopia was one dominated by this social group. In addition, it can be argued that Galton was essentially making a takeover bid for the dominion traditionally ruled by religion by explanations of the human condition couched wholly in naturalistic terms. The connection with statistics came with the need to find some means of demonstrating the relationship between parent and offspring generations. From this fol-

lowed the invention of methods for correlation and regression analysis essentially different from anything that had gone before.

Pearson, although a socialist, shared many of Galton's social assumptions and goals, and like Galton his advances in method were made in response to the needs of biological analysis based on eugenic models. Thus, while very much an individual, he expressed a philosophical viewpoint that represented one possible form of ideology for the professional middle class. From a very different political perspective Fisher continued much of that emphasis.

The connection between statistics and eugenics was undermined in the decade or so before the First World War by three factors: the rise of Mendelian biology, the emergence of statisticians whose primary interest lay outside human eugenics (such as Gosset of Guinness's brewery), and the apostasy of some who moved through Pearson's "biometric" school to other concerns (such as Major Greenwood). To some extent Mackenzie might be criticized for not seeing that this convergence of circumstances marks the effective end of the phase of statistics he is interested in and that it was unnecessary to continue the work to 1930 (particularly given the lack of cooperation from the University of Adelaide, which refused access to Fisher's papers).

M. J. CULLEN
House of Representatives
Wellington, New Zealand

RICHARD MORAN. *Knowing Right from Wrong: The Insanity Defense of Daniel McNaughten*. New York: Free Press. 1981. Pp. xiii, 234. \$15.95.

The trial of Daniel McNaughten is one of the most famous in the annals of psychiatric jurisprudence, because it led to the promulgation of the much discussed (and cursed) McNaughten Rules, a set of guidelines for judging insanity pleas. The man who gave his name to the Rules was a Scottish shopkeeper who in 1843 assassinated the private secretary to the British prime minister, Sir Robert Peel (though Peel was almost certainly his intended victim). At his trial, McNaughten was acquitted on the grounds of insanity when the defense produced several medical witnesses who testified that his crime was the result of a delusion that Peel and the Tories were persecuting him. The verdict produced a public furor and led the House of Lords to call upon the chief justices to state exactly what the laws respecting the insanity plea were. The judges' replies became known as the McNaughten Rules, which remained the sole test of criminal responsibility in England until 1957 and are still used alone or in conjunction

with other tests in 38 American states. The Rules are often referred to as the right and wrong test, because they require that a defendant be incapable of knowing right from wrong in order to be acquitted on the grounds of insanity.

Since 1843 jurists and psychiatrists have expounded endlessly on the pros and cons of the Rules. The trial of McNaughten himself, however, has never excited much comment. Scholars have generally assumed that McNaughten was insane. In *Knowing Right from Wrong*, Richard Moran challenges this assumption with a detailed investigation of the trial that for the first time places it in its historical context. Moran argues that the political, economic, and social climate of the time, as well as the Scotsman's own experiences, provided a rational motive for the crime. In the 1840s Britain was suffering from severe economic depression and the strains of rapid industrialization and urbanization. Many individuals and organizations, like the Chartists and the Anti-Corn Law League, blamed Peel and the Tories; they called for violent resistance against the government and the removal of Peel by one means or another. Given this context, Moran believes McNaughten may have been trying to accomplish what others were hoping for or contemplating. There is some evidence that the government may have had documents linking McNaughten to a political conspiracy involving Chartists or Leaguers; moreover, when arrested he was carrying a bank note for 750 pounds, a princely sum in those days. None of this evidence was brought forward at the trial.

Moran claims that McNaughten's delusion of Tory persecution may have had a basis in fact. McNaughten was a Chartist who had taken part in antigovernment activities; he may actually have been harassed by the local Tory machine in Glasgow. Although there is little direct evidence of harassment, it was common enough at the time, and a person like McNaughten would have been a logical target. Moran believes that such evidence would have surfaced at the trial had the prosecution chosen to contest the defense case.

Why did the prosecution concede? Moran argues that the solicitor-general, Sir William Follett, could have secured a guilty verdict, but he would have been forced to contend that McNaughten had been motivated by a desire to redress personal or political grievances against Peel and the Tories. This would have necessitated embarrassing revelations about the government's activities and unpopularity. Instead, Follett claimed that McNaughten was suffering from "partial insanity" on the subject of politics but was responsible for his actions. This was a more plausible tactic than it would be today, but Follett presented his arguments in such a confusing manner that they were probably lost on the jury.

Moran does not claim that the resulting insanity verdict was the result of a plot to discredit McNaughten—most of the authorities in fact wanted him hung—but it did have that effect, for it rendered his act a meaningless aberration. Moran's book is in large part an attempt to rescue McNaughten from the condescension of history and to show that he was motivated by something more than a crazy delusion. In this the author largely succeeds, but the lack of documentation on crucial points leaves the exact nature of McNaughten's intentions and mental state a mystery.

Moran's thesis is stimulating and provocative, and his attempt to place the McNaughten case in historical perspective is long overdue. Unfortunately, at times the protagonist and his case become lost in the details of early Victorian history; this material could have been summarized more briefly. The greatest weakness of the book, however, is the author's attempt to generalize from the McNaughten case the need for Anglo-American jurisprudence to recognize the legitimacy of a political defense for criminal actions. Moran's argument for such a defense is in some ways compelling, but he downplays the fact that it would make assassination more attractive to those who wished to make a political statement and that no government is likely to encourage a measure that conceded that such a crime might be justifiable.

PETER MCCANDLESS
College of Charleston

JACK MORRELL and ARNOLD THACKRAY. *Gentlemen of Science: Early Years of the British Association for the Advancement of Science*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xxiii, 592. \$49.95.

Jack Morrell and Arnold Thackray base their history of the formative years of the British Association for the Advancement of Science on a more thorough and extensive canvass of unpublished documents, mainly correspondence, than has ever been effected for any scrutiny of British science in the nineteenth century. Indeed, I am not aware of any study of a scientific institution that draws so largely on the private exchanges of the leading figures. The work began with the discovery of a wealth of information preserved in the papers of William Vernon Harcourt, and it expanded to a survey of all the pertinent collections in the United Kingdom.

The result is a very interesting and informative book, highly detailed and entirely reliable. Beyond saying that, which is the essential matter, I am puzzled to know how to characterize it briefly. The authors' main purpose is to exhibit the institutionalization of science as a component of general culture

in industrialized society. One would not quite wish to state that science had been esoteric and amateurish beforehand and that it became intrinsic and professional in consequence of the activities of the British Association, but perhaps those two sets of poles may be taken as a reviewer's oversimplification of what was at stake. Morrell and Thackray themselves turn to Coleridge for assistance in generalizing their material and make great play with the notion that the membership of the Association constituted a scientific clerisy.

Like all historians of nineteenth-century Britain, Morrell and Thackray are confronted with many complexities, foremost among them being the persistence of aristocratic forms and patronage in a materially and culturally bourgeois environment. They give us a great deal on the organizational dilemmas, the internal scientific politics, and the ideologies involved. Perhaps it is unreasonable to want more, but there are two large and possibly related questions that I wish they had addressed. First, although the British Association arose in the context of contemporary disputes about the reality of a decline in British science in the early nineteenth century, we do not learn what the authors themselves think about that issue. Neither, secondly, do we learn what they think about the responsibility of the British Association for the undoubted vitality of British science from the 1830s onward.

In my view the scientific fields where the content—and the contrast with what went before—was most impressive were mathematics, theoretical and experimental physics, and (thanks to Darwin) evolutionary biology. But the leaders in the harder sciences figured scarcely at all in the affairs of the British Association. Its annals are peopled largely with naturalists, geologists, and representatives of the softer sciences, easier to popularize and talk about, but less clearly important to an industrial society.

Is that not odd? Or is it?

CHARLES C. GILLISPIE
Princeton University

AVNER OFFER. *Property and Politics, 1870–1914: Land-ownership, Law, Ideology, and Urban Development in England*. New York: Cambridge University Press. 1981. Pp. xviii, 145. \$55.00.

Just as no one disputes that, for centuries, land and its ownership was the central determinant of property and status in Britain (as elsewhere), so historians have almost invariably ignored landed property and its ownership as a central source of debate and division in the postindustrial era, reserving it for the literature on the great agricultural landowners and the landed estate. Avner Offer's impressive and

important *Property and Politics, 1870–1914* reminds us just how central land and its ownership remained to British political opinion and to property owners, long after industrialization. Offer takes as his subject all landed property, urban and rural, and all of those who owned, leased, or rented any house or land: the entirety of the population. He demonstrates with great skill and extraordinary erudition how conflict over land tenure or profits and rents from land or housing, or among the various professional groups (above all solicitors) that profited from the sale of land, formed one of the central themes of political debate in late Victorian and Edwardian England. Such conflict most commonly arose at the local level, but it occurred nearly as often at the national level in broad questions of policy, taxation, and party politics.

All of this, it is safe to say, is *terra incognita* for most historians, and the author deserves considerable credit for rescuing this subject from near-complete oblivion as well as recreating the issues and the passions they generated so well. Offer demonstrates his mastery not merely of the literature of contemporary pressure groups and political advocates, from Henry George to the Liberty and Property Defense League, but also of economic history, economic theory, and legal history in a manner equaled by few recent historians. His knowledge of taxation sources is particularly impressive. Offer shows the shifting and often surprising evolution of party attitudes toward land and property as well as toward local revenues that depended so heavily on property rates. His initial discussion of solicitors, half of whose income typically came from conveyancing, and of the crisis in income and job opportunities evident during the Edwardian period—and paralleled more generally in property values at the time—is most important and ought to lead to a significant second book on the crisis in the British middle class during the Edwardian period (promised by the author). Offer shows the importance for the British left of Henry George, and his handling of the cultural and ideological importance of the land theme is equally stimulating.

The book is not without its blemishes, which are largely the vices of its virtues. In part derived from Offer's dissertation, the middle section of the book—largely dealing with the pressure groups for rating, land tenure, and reform that emerged after 1870—is probably far too detailed for any but the specialist, while the brilliant first chapter, on solicitors and their vicissitudes, is out of key with much of the rest of the book. The author's (or perhaps the publisher's) penchant for abbreviating commonly employed terms is annoying and reaches the ludicrous with "SH" for "small holdings." More generally, the subject matter of the book is so vast and full of ramifications in every quarter that the thread of

continuity between one section and another often seems tenuous, the boundaries of discussion ill defined and arbitrary. There is, for instance, no real discussion of the incomes of large land and property owners in this period, and other seemingly relevant subjects are not touched.

These are, however, small points, and overall it is clear that Offer has produced a work of very considerable importance and great originality. It is no mean feat for a historian to rescue a subject from near-total neglect, but additionally Offer has given it the expert and comprehensive treatment it clearly merits. In the process he has virtually added a separate and novel area of study to British history in the period 1870–1914.

W. D. RUBINSTEIN
Deakin University

J. MORDAUNT CROOK. *William Burges and the High Victorian Dream*. Chicago: University of Chicago Press. 1981. Pp. 454. \$55.00.

STEPHAN TSCHUDI-MADSEN. *Restoration and Anti-Restoration: A Study in English Restoration Philosophy*. 2d ed. Oslo: Universitetsforlaget; distributed by Columbia University Press, New York. 1981. Pp. 164. \$19.00.

William Burges, the “Pre-Raphaelite architect” as J. Mordaunt Crook calls him, receives his due at last in a book that is entirely apt for its subject: scholarly, massive, bulky, rich in incident and varied rhythm, and with a profusion of detail and anecdote. Burges certainly needed an adventurous biographer and commentator, being himself an architect and designer of extraordinary learning, bold and imaginatively synthesizing in his repertoire of style and syntax, inventive, and “truly obsessive.”

His architectural work included the winning designs for Lille Cathedral (1856) and for the Crimea Memorial Church in Constantinople (1856–61), both of which were unexecuted; unsuccessful designs for the decoration in St. Paul’s Cathedral (1870–77) and for new Law Courts in the Strand (1866–67); ambitious, but only partly executed, plans for a four-quadangle Trinity College in Hartford, Connecticut (1873–82); above all, ambitious, executed, and successful designs for churches such as St. Mary’s, Studley Royal, Yorkshire (1870–72) and private residences for the third marquess of Bute at Cardiff Castle and Castell Coch (from late 1860s onward) and for himself at Tower House, Kensington. Besides these and other buildings and improvements, there was an astonishing stream of designs for sideboards, caskets, bowls, ironwork, copes, goblets, and other objects. Some of these, together with examples of his drawings, have been

on display recently at the Victoria and Albert Museum.

Crook’s book sets detailed examinations of Burges’s various works in the context of Victorian Gothicism, especially Pre-Raphaelitism. His skills with “neat categorization” effect dense, well-documented introductions to the “High Victorian Dream” that provides his subtitle: there are synoptic accounts of “Romantic Tories,” “Romantic Socialists,” and “Romantic Artists”; also, approximately forty pages on the battle of styles—including battles between architecture and engineering, archaeology and “invention”—are a model of compression. If one has reservations, they are that, firstly, such synoptic tours de force can only, in fact, mean much to persons already familiar with the field, and that, secondly, subtleties sometimes elude even Crook’s agile and inventive aphorisms. He himself notes how “Carlyle’s political philosophy defies neat categorisation,” and Ruskin’s diversities also escape his net of pithy résumé; so, although there is a bracing air about so much of the writing (“Billy Burges first cut his teeth as a budding Corbusier of Gothicism”), it just occasionally inhibits probing analysis.

Similar objections may be laid against the sections that survey Burges’s work in “Renaissance,” “Gothic,” “Feudal,” and “Fantastic” architecture and decorative arts. The detailed descriptions and commentary are exemplary, and together with the chronology of Burges’s work and publications, plus the marvelous photographic survey of all aspects of his career, they make this book a treasure house of detail that will be mined by Victorianists for years to come. Yet there is (finally) something a little evasive about such phrases as “an out and out Ruskinian” or “Pre-Raphaelite” to describe Burges; one needs some more probing analysis of this “obsessive” genius. Crook scrupulously avoids any psychological interpretations, but by the time he reaches Burges’s self-apotheosis, the “Palace of Art” of his own Tower House in Melbury Road, Kensington, such perspectives are perhaps inevitable. The allusive quotations from Tennyson, used so tellingly throughout the book, suggest cultural as well as psychological explanations of the work of a man who drank claret out of rock-crystal and ate whitebait off cloisonné and who built in Cardiff Castle or planned at Knightshayes in Devon composite structures otherwise denied to the Victorian world.

Burges was involved most intelligently, as Crook discusses, in restoration work. The second edition of Stephan Tschudi-Madsen’s *Restoration and Anti-Restoration* does not mention him—a telling indication of our previous ignorance of “soul-inspiring Burges” (in Lord Bute’s phrase). But the study is a lucid, if unstrenuous, survey of theories of restora-

tion, relying generously on quotations from relevant works and including, as an appendix, chunks of major texts from Ruskin, Morris, RIBA papers, Viollet-le-Duc, and W. J. Loftie. The illustrations are also particularly useful.

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G. E. MINGAY, editor. *The Victorian Countryside*. In two volumes. Boston: Routledge and Kegan Paul. 1981. Pp. xvi, 363; ix, 367–702. \$85.00 the set.

Almost a decade ago Routledge and Kegan Paul brought out in two volumes *The Victorian City: Images and Realities*, edited by the Anglo-American team of H. J. Dyos and Michael Wolff. This was a lavish production, to say the least: a dramatic celebration of a newly established discipline, that of urban history, appropriately unfolded with full interdisciplinary *éclat*. Now we have the companion work, also in two volumes, on the Victorian countryside: equally splendid in appearance, less interdisciplinary perhaps (for which some historians may be grateful), and sponsored once more by Routledge and Kegan Paul.

That the countryside had to wait longer for its celebration than the city is a bit odd. Agrarian historians started to work earlier than urban historians, and, as the thirty-eight-page (but by no means exhaustive) bibliography in *The Victorian Countryside* reveals, they have been very far from idle. Indeed, their work—primarily in British universities—has in the past thirty years been both voluminous and illuminating, a veritable torrent of publication that only the most assiduous of specialists can keep up with. For the scholarly generalist and the general reader—each likely to be put off both by overmuch detail and by a widely shared conviction that agrarian history is trivial and boring—*The Victorian Countryside* serves admirably as a guide through the thickets of research and as a convincing demonstration that agrarian history deserves a better reputation.

As editor, G. E. Mingay has made this possible and deserves much credit; editors are a much put-upon race. Mingay has brought together forty expert contributors who have written forty-five articles, each 4000–5000 words in length, accompanied by 170 illustrations and photographs of great variety and often of impressive eloquence. The articles are arranged under five main headings: the land (landscape, religion, and literature); agriculture (farmers, mechanization, and farming societies); country towns and country industries (professions, tradesmen, and craftsmen); landed society (aristocracy, gentry, land agents, social control, and sports); and

laboring life (field work, leisure, crime, and poverty). Plainly little has been overlooked, although one misses such subjects as local government, rural banking, and perhaps a comparison of British and continental countrysides. Also there might have been more mention of the Scottish, Welsh, and Irish countrysides over which Victoria ruled as formidably as she ruled over the English countryside. And finally, although one hesitates to add to the grievous burdens of an editor, the introduction might have done more to tie loose threads together—or, to put it another way, to knock some contributors' heads together.

Of the many good things in *The Victorian Countryside* I would suggest the following, going on the assumption that most readers will rightly prefer to make a selection rather than read through the two volumes from start to end. Since farmers have been strangely ignored by historians, I recommend strongly the two articles by B. A. Holderness in which he boldly attempts to portray something of the society of these largely forgotten men. In the same sense of opening up new social vistas, C. W. Chalklin is good value. "In the Victorian period, as in earlier centuries," he writes, "the country town was the basic English urban type" (vol. 1, p. 276). Barbara Kerr on the country professions also merits attention: this is the world of George Eliot's *Middlemarch*, the world of the clergy, apothecaries, lawyers, and surveyors.

Many of the essays on the world of the rural laborer are notable, providing a salutary corrective to the stubborn but wrongheaded notion that the English countryside was a Garden of Eden. Note especially the essays by Alun Howkins, Enid Gauldie, Anne Digby, and Michael Winstanley. The first dwells vividly on the laborer's unremitting toil in all weathers, from the sleet and mud of winter ploughing to the endless hours of harvesting; the second on the overcrowded slum housing, "a rural people living in conditions of misery and squalor throughout Victoria's reign" (vol. 2, p. 533); the third on a level of wages still too low in 1900 to provide a family a healthy life; and the last, perhaps the most moving of all, based on oral interviews with Kentish laborers born before 1914, on the authentic voice of the laboring poor in all its pathos, simplicity, and dignity.

Last but not least there is F. M. L. Thompson's essay on landowners and the rural community, in some ways perhaps the most original contribution of the whole collection. This highly fastidious and accomplished historian has recently (in the May 1981 issue of the *Economic History Review*) taken a close and heretical look at the fashionable notion of social control, and his essay in *The Victorian Countryside* furnishes a special application of his more

general views on the subject. In brief he invites us to share his scepticism, arguing that social control "has never been exercised with unanimity and agreement among the various agents of authority, nor with complete effectiveness, for otherwise social changes could not have occurred." Many English landowners, he concedes, consciously and effectively worked such agencies of social control in the countryside as the church, schools, and the police, but "many more," he goes on, "the silent majority as it were, were not sufficiently active, imaginative, responsible or assertive to try to use their power and position to influence the rural community in any systematic way" (vol. 2, p. 458). It is therefore "an open question" (vol. 2, p. 473) whether open and closed parishes tell us much about the incidence of landed paternalism. We must rethink the question of what made rural society hang together; indeed, we must rethink what we mean precisely by that cant phrase, social control.

If *The Victorian Countryside* has some flat and dreary places, it also has—as in Thompson's and in many other essays—its high and bracing places. All in all, it reminds us—if we need the reminding—that to explore English rural history is both demanding and exciting, both instructive and pleasurable.

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MARK GIROUARD. *The Return to Camelot: Chivalry and the English Gentleman*. New Haven: Yale University Press. 1981. Pp. 312. \$35.00.

Mark Girouard's fascinating archaeology of the nineteenth-century British mind examines how a revived interest in chivalry broadly influenced the ways Englishmen thought and acted from the early decades of the nineteenth century until the end of the Great War. *The Return to Camelot* begins with a glance at the gentlemanly behavior of those who perished in two disasters that captured popular imagination in the early years of this century: the sinking of the *Titanic* and Scott's ill-fated expedition to the North Pole. The first chapter well represents Girouard's many strengths in writing this kind of social and cultural history, for in it he seizes upon representative social phenomena, which he then uses to tell a story that embodies major ideas and attitudes. Clear-eyed and unsentimental, Girouard reveals how deeply popular myths shaped the perception and memory of such events; and yet, despite all his wit and irony, he sympathetically enters the imagination of the age in order to help us understand its fundamental ideas and ideals.

After chapters that examine medieval chivalry, its Renaissance survival, and its rejection by eighteenth-

century progressivism and rationalism, *The Return to Camelot* demonstrates the revival and reinterpretation of the chivalric ideal of conduct in Walter Scott, Kenelm Digby, and Tory Radicalism and shows the importance of the famous Eglinton Tournament, Victoria and Albert, and a host of middle-brow artists and authors. Having shown how the chivalric ideal entered the thought of the upper classes, Girouard discusses its impact upon education, imperial administration, sports, and scouting, all of which discussions culminate in a well-told tale of what happened when these ideals encountered the horrors of the First World War. In addition, within these strands of his story he interweaves the related revival of interest in Arthurian matter and late-nineteenth-century courtly love.

Girouard throughout employs an essentially Ruskinian approach: that is, one that uses a civilization's artifacts—particularly its architecture, painting, design, and other visual arts—as ways into its mind and soul. To succeed, such an approach requires that the interpreter fasten upon precisely the right details, which he then must demonstrate are representative and even metonymic. Since much of the evidence he adduces is visual, his approach requires extensive high-quality illustration. As his earlier works, *Life in the English Country House* and *The Victorian Country House*, demonstrate, Girouard is a master at producing beautiful, lavishly illustrated volumes that make contributions to cultural history and yet also make handsome "coffee-table" or gift books. Part of the credit for such successful ventures must go to Yale University Press, which has been willing to take the financial risk that these volumes will interest large groups of readers (and purchasers).

Girouard has a fine eye for detail and an equally fine ear for anecdote; and at the same time he wears his extensive knowledge—and that of others—lightly. Unfortunately, this generally excellent and often delightful volume makes a half-hearted and rather silly display of scholarly apparatus when virtually none is in fact present. All too often one turns to the endnotes to discover that they contain nothing more than the title of a quoted work, something that could easily have been included in the text. The frequent failure to provide the page and edition used is particularly odd when he quotes Ruskin and other authors whose works have long been available in standard editions. Even more bothersome, to be sure, is Girouard's failure to mention his many honorable predecessors, such as Alice Chandler, whose *A Dream of Order* (1970) anticipates much in *The Return to Camelot*. It is not so much that Girouard fails to give credit where credit is due as that many readers would like to have his advice about what to read next. Despite my expressions of minor annoyance, I must emphasize that Girouard

has again written a useful, engaging, and frequently delightful book.

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without fully appreciating the background and underlying causes of that rise and decline.

HOLDEN FURBER
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PETER LOWE. *Britain in the Far East: A Survey from 1819 to the Present*. New York: Longman. 1981. Pp. 264. \$25.00.

In this brief survey of British policy in the Far East since 1819, Peter Lowe has devoted most of his attention to the years since 1900. Less than a third of his account is devoted to the nineteenth century; the founding of Singapore is taken as a starting point, with only the briefest reference to the reasons for it. Three chapters describe the period before 1898, stressing the opening of China and Japan, the treaty-port system, and the development of "formal and informal empire" (p. 41). Three more discuss the Anglo-Japanese alliance, the Chinese Revolution, and the impact of the First World War in East Asia. We then pass on to the five chapters devoted to the British role in East Asia since 1919. These form the heart of the book, leading the reader through the complexities of the Japanese penetration of China and the European and American reaction to it and culminating in a brief account of World War II and its aftermath in the Pacific.

In short, this book is an admirable survey of facts with too little attention to their underlying causes. It does not supply the broader background that the student or general reader requires for his understanding. The needs of the American reader have not been kept sufficiently in mind. The American college student without an elementary knowledge of twentieth-century British politics and colonial government will be at a loss to understand many of the author's terms of reference. It is hard for those of us who lived through the 1940s to realize that most of the students for whom such books as this are primarily written will not at once know what is meant by "the destroyers-bases deal" of September 1940 (p. 169). Even the British reader will occasionally be puzzled. Such terms as *Tsungli Yamen*, *likin*, and *Gaimusho* are not sufficiently explained, and names of minor officials—British, Japanese, and Chinese—appear in bewildering numbers.

Lowe is at his best when describing the complexities of Japanese penetration of China in the 1920s and 1930s, but even here the reader needs less detail and more interpretation. The survey would have benefited greatly by a broader approach. India, Burma, and Malaya are too far in the background and are brought into the story only when they cannot possibly be left out. One lays the book down with a knowledge of the rise and decline of British influence and power in the Far East but

PHILIP M. TAYLOR. *The Projection of Britain: British Overseas Publicity and Propaganda, 1919–1939*. New York: Cambridge University Press. 1981. Pp. xv, 363. \$44.50.

Fifty years ago Sir Stephen Tallents's phrase, "the projection of England," attracted very considerable attention. But a systematic study of agencies and procedures designed "to project national personality" had to await our day. It is high time, suggests Philip M. Taylor, as he launches into his "pioneering study of a little-known aspect of British foreign policy between the wars" (p. vii). He insists that his is but a preliminary examination; surely it will be the starting point for all further investigation of propaganda in Britain and its impact on public opinion. Taylor's presentation is based on solid and almost exhaustive research in the private papers of individuals as well as the files of the innumerable agencies concerned.

Taylor's method is, inevitably, thematic rather than consecutive or chronological. He presents a mass of detail, by necessity often repetitious, that defies analysis in a brief review. First he examines political agencies, especially the Foreign Office with its news department, and the gradual transition from mere publicity to "propaganda." It was very often a halting process because of official opposition, especially in the Treasury, and because of differences among the newsmen, particularly Arthur Willert and Rex Leeper, who often worked at cross purposes with their political masters.

Then Taylor turns to "commercial publicity" through the news department of the Foreign Office and such agencies as the Empire Marketing Board. Study of "cultural propaganda" directs attention to the British Council (established 1934), especially under Sir William Tyrell, with its use of films and foreign language broadcasts of the BBC and the activities of the Travel Association. The British Council, in Taylor's summation, was "the first genuinely institutional response in Britain to the increasing importance of propaganda as a factor in peacetime international affairs" (p. 181).

The early chapters prepare the way for detailed study of "psychological rearmament" in the period 1935–39. How to compete with other countries whose governments entirely control the press; that was the major question. Unity among the very many agencies was essential. Centralization of authority seemed to be the answer, and this was achieved, if only to a degree, in the Vansittart Committee for

the Coordination of British Publicity Abroad, established in 1938. This led to the first major discussion in Parliament since 1918 of British propaganda in the interwar years and reflected great interest (if not achievement) at top levels of government. Taylor concludes with an examination of preparations for propaganda in the event of war and the implication for agencies already in existence.

This book has certain limitations, perhaps inevitable and generally recognized by the author. A myriad of agencies calls for attention, and assessment of their value is difficult both for author and reader. At times Taylor moves so rapidly that he leaves the reader far behind. But it is more important that Taylor is acutely aware that "publicity" and "propaganda" are highly controversial, both as to meaning and as to impact, even "un-English" to some. Even for World War I, which has been considerably worked over, there is no consensus as to the significance of "British propaganda." But Taylor himself has remarked: "At least one thing is clear. During the interwar years, publicity became an established fact of British political and diplomatic life" (David Dilks, ed., *Retreat from Power* [1981], I, p. 63). Other historians will no doubt take off from there.

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ELEANOR M. GATES. *End of the Affair: The Collapse of the Anglo-French Alliance, 1939-40*. Berkeley and Los Angeles: University of California Press, 1981. Pp. xviii, 630. \$28.50.

End of the Affair: The Collapse of the Anglo-French Alliance, 1939-40 by Eleanor M. Gates is a long and heavily documented account of the breakup of the Entente Cordiale during and immediately after the defeat of France in June 1940. The book runs to some four hundred pages of text (plus almost forty of appendixes), supported and embellished by about one hundred and thirty pages of notes. The bulk of the work is a blow-by-blow account of the deliberations of the British and French governments and the deliberations conducted between them, as both sides realized that France was defeated, and drew the conclusion that this defeat meant that the two sides would go separate—and finally hostile—ways. The story, played against the backdrop of Nazism triumphant, is an impassioned one: the reader watches the actors—particularly the French leaders—struggling futilely against ineluctable defeat, then resigning themselves to it as the British leaders pick up the burden of resisting Adolf Hitler and cast about for ways to squeeze the last ounces of utility out of the dying Franco-British alliance. Although at times the book bogs down in the twists and turns of evidence, Gates succeeds in

conveying much of the atmosphere of the times; certainly she makes every effort to be impartial.

The book does have flaws, however; it makes use of British archives and private papers but ignores similar materials from the French side. These French materials are available; some of them have been for a number of years. Inevitably, Gates's choice of sources colors her interpretation, particularly her interpretation of the critical period up to the Dunkirk evacuation. She treats this whole period only as background, and that is unfortunate since it was during this period that British decisions weakened the alliance (as she notes). In interpreting these British decisions she inevitably tends to follow the British line: had she read the French documents, she might well have seen things differently. Further, her narrow focus on the collapse of the Entente leads her to all but ignore the small members of the impromptu Allied coalition that formed on May 10, 1940: Belgium, the Netherlands, and tiny Luxembourg. Allied hopes for victory in 1940 rested on the French commander, General Maurice Gamelin, and his plans for welding the whole Allied coalition together on a viable front to resist the Wehrmacht. By largely ignoring the small countries, by focusing only on Franco-British relations, and by relying too heavily on British archival sources, Gates misses the critical point of Gamelin's plans. She also fails to use the excellent sources from the small countries, such as General Raoul Van Overstraeten's diary or the voluminous information of the Dutch and Belgian parliamentary investigations and official histories. Indeed, she does not even use N. H. Gibbs's volume of the British official histories, which flays British policy during the thirties; nor key recent publications like Colonel Paul de Villelume's diary (Villelume was one of Paul Reynaud's principal advisers); nor recent secondary works like those of Jean-Baptiste Duroselle, Henri Michel's *La défaite de la France*, Robert J. Young's *In Command of France*, and my own *Divided and Conquered*. Had the author consulted these works, I suspect her interpretations might well have changed.

In conclusion, *End of the Affair* seems to me a well-written and unbiased account, but one that focuses too narrowly on the spectacle of the collapse of the Entente Cordiale and that suffers from overuse of British archival sources to the exclusion of French and other materials. The defeat of 1940 was a disaster for all of Western Europe—for all of Western civilization; this is a point that *End of the Affair* largely misses.

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CHRISTINA LARNER. *Enemies of God: The Witch-Hunt in Scotland*. Foreword by NORMAN COHN. Baltimore:

Johns Hopkins University Press. 1981. Pp. x, 244. \$17.50.

The scope of this thoughtful and scholarly book is much wider than its subtitle suggests. The first chapter provides a critical survey, intelligent and lucid, of the scholarly work on witchcraft of the last twenty or so years, though with two notable omissions: Robert Mandrou's *Magistrats et sorciers en France au XVII^e siècle* (1968) and Carlo Ginzburg's *I Benandanti* (1966). Throughout her book Christina Larner reflects critically on her own presuppositions and methods, and, in her final chapter, which places the Scots' witch hunt in a European perspective, she tackles the great, general questions posed by the European witch craze: Why did it reach its climax when it did, in the age of Bacon, Galileo, and Descartes, rather than in the "autumn of the Middle-Ages"? Why did some regions, such as England, most of Spain, and Italy, escape any major witch hunt? Can one discover underlying causes or necessary preconditions, religious, political, social, or economic, for an epidemic of witch accusations?

An affirmative and satisfactory answer to the last question would, of course, also answer the first two. It would be misleading to give a brief summary of Larner's sensibly cautious and carefully qualified answers to these questions, but I will mention two that seem to me indisputably true. First, the tradition of witch beliefs is remarkably tenacious and long-lived and is a necessary precondition for any large-scale witch hunts; where the tradition has never been established, as in southern Spain and Italy, witch hunts do not occur, even though other conditions are favorable to them. Second, a correlation can be drawn between the presence or absence of major witch hunts and those areas within and without the spheres of Roman law or canon law, both of which produced an inquisitorial system of legal procedure supplemented by the use of torture to obtain evidence. As Larner points out (p. 197), "areas on the periphery of this system, such as England, Russia, Denmark and Finland, experienced milder hunts with less reference to diabolism and conspiracy."

The witch hunt in Scotland can be ranked with the major epidemics of Europe: between 1590 and 1700 over one thousand people were executed for witchcraft and about three thousand accused. In contrast to England, the witches' Sabbath figured prominently in these accusations. But in contrast to European Sabbaths, as Larner almost regretfully observes (p. 200), "there was very little actual Devil worship or other forms of inverted Christian ceremony. Reported sexual orgies, other than private copulations with the Devil, were relatively rare, and baby-eating was almost unknown."

The author examines carefully the legal mecha-

nisms of accusation, the social status of accuser and accused, and the political motives behind the witch hunts. These last, she argues convincingly, were of major importance in Scotland as in Europe: new nation-states, anxious to prove their legitimacy, demanded a high level of social and political orthodoxy among ordinary people, and, since witches represented every kind of popular deviance, dissent, and rebellion, their prosecution provided the ruling classes with a simple and effective means of asserting social, religious, and political control. In this connection it is a pity that we are not given a full account of James VI's hunt of the Berwick witches in 1591, which, according to Larner (p. 198), was of cardinal importance in establishing the tradition of witch hunting in Scotland. In general, I would have liked more accounts of particular trials and groups of trials, but these might have produced an excessively large book. In any case, this is a fine study that makes a very important contribution to our knowledge of seventeenth-century witchcraft, and all students of this period should read it.

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F. J. MCLYNN. *France and the Jacobite Rising of 1745*. Edinburgh: Edinburgh University Press; distributed by Columbia University Press, New York. 1981. Pp. 277. \$26.50.

This book is, in general, a very good study of French plans and attempts to launch an invasion of Great Britain in the period from 1743 to 1746, the aim of which was to overthrow the house of Hanover and restore the exiled Stuart family to power.

Although long urged by Jacobites to do something of the kind, the French court (that is, Louis XV, his ministers, and his military advisers, all with diverse inclinations and conceptions of French interests) only became seriously interested in the project in the autumn of 1743. Charles Stuart, the elder son of the Old Pretender, went to France to join an expeditionary force, but that force failed to get away for various reasons, all detailed here. Later, Charles lingered in France, in spite of discouragement by French ministers. His independent venture into Scotland in 1745 rekindled the almost dead proposal, but communications were slow and difficult, and French preparations could not be coordinated with Charles's march into England. Moreover, the French authorities disagreed among themselves. The little help actually sent did not have much influence on the outcome, and a larger assisting force never departed. In the end, rescue of survivors was all that was possible.

F. J. McLynn did his research at the Royal Archives in Windsor Castle, the Public Record Office,

the French Ministries of War and Foreign Affairs, and elsewhere. He did not neglect printed sources or secondary works pertaining to his subject. He might have spared his readers trouble by reversing the order of his first and second chapters and introducing the French ministers, who are important to his story, on first mention. He does make clear the shifting stands of the Comte de Maurepas, the Duc de Richelieu, Cardinal de Tencin, and others while the invasion was being considered and planned, as well as the administrative tangles that had to be unsnarled for success.

Among factual errors are McLynn's identification (p. 12) of John Gordon of Glenbucket as a "chieftain"—a word that must be used carefully in dealing with Scotland—and his statement (p. 39) that Philip V of Spain married a daughter of Louis XV. (Such a daughter would have been Philip's grand niece.) Once he needlessly raises an *argumentum ex silentio* on whether the French requested the Young Pretender's journey to France. Sometimes McLynn does not seem at ease with noble titles. His index is selective, omitting, for example, any mention of Gordon of Glenbucket.

These are not grave defects. McLynn's book will take its place in the growing body of dependable literature on the Jacobite movement.

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BLANCHE M. TOUHILL. *William Smith O'Brien and His Irish Revolutionary Companions in Penal Exile*. Columbia: University of Missouri Press. 1981. Pp. xii. 269. \$24.00.

It is encouraging to find that a management-level administrator can remain an active scholar. Blanche M. Touhill is Associate Vice-Chancellor for Academic Affairs at the University of Missouri, St. Louis, and has written a book about a mid-nineteenth-century Irish patriot who attempted to step into the leadership vacuum in Irish national politics during the interregnum between the decline of Daniel O'Connell in the mid-1840s and the reappearance of a broadly based, indeed transatlantic, nationalist movement that produced the Home Rule effort of the 1870s and 1880s. William Smith O'Brien was a Protestant Cambridge graduate who represented Limerick County at Westminster. He was an acknowledged leader among a small group of Dublin-based, respectable young Irish nationalists who split with the aging O'Connell on the issue of the ultimate right of the Irish people to resort to armed rebellion if the British government refused to yield to Irish demands for legislative independence, the return of a parliament to Dublin. This alignment was known as Young Ireland and evolved in 1847

into the Irish Confederation, with elements in America and, more importantly, the northwest of England. By later nationalist standards, the objectives of the Confederates were modest (an independent Irish parliament and the retention of the crown connection), but in 1848, the year of revolutions, imminent arrests prompted O'Brien and a very small number of lieutenants and supporters to lead into insurrection an Ireland only beginning to recover from the Great Famine.

The effort ended in a bloodless farmhouse confrontation in Tipperary and resulted in the arrest of O'Brien and several other Confederate organizers. O'Brien and his friends, though guilty of treason, were sentenced to terms of "transportation," or exile, in the Australian penal colony that is now Tasmania. The body of Touhill's work is devoted to a sympathetic narrative account of the Confederates' lives in exile on very favorable, gentlemen's terms that largely excluded hard labor or confinement. The author devotes particular attention to O'Brien's persistent effort to keep the nationalist question before the Irish public through his attempts to achieve better penal terms and a pardon.

The O'Brien who emerges from Touhill's research in Irish and British manuscript collections and a variety of contemporary newspapers was a man of stern principle and integrity who was determined to be an Irish nationalist leader. He could well have thought that his efforts during 1848 had not succeeded in achieving that status because, in the midst of famine Ireland, he found himself virtually without active support, and, despite Touhill's confidence, the degree of even sympathetic support is unknown. Indeed, O'Brien and his cause may have had a larger number of adherents among the Irish in Britain and certain English Chartist branches. Although O'Brien certainly did not become the pivotal leader in Irish nationalist politics, he did succeed in making his own exile and that of his colleagues a focus for nationalist sentiment during the 1850s, evidenced by the momentum that sustained a successful lobbying campaign on their behalf in Australia, North America, and Ireland.

The style in *William Smith O'Brien* is concise and factual, rather in the style of a nineteenth-century newspaper. Although the University of Missouri Press has produced an attractive volume, there are lapses in editing that are troublesome, including antiquated usage ("revolutionist" for revolutionary) and incorrect words ("ameliorization" for amelioration on page 10, "immigrating" for emigrating on page 196). There are topics—admittedly not central to the principal theme of the book—that would enhance the story for many readers. For example, did O'Brien or his colleagues come into contact in Australia with the English Chartist exiles of 1839?

Also, we learn very few details of life and society in nineteenth-century Australia or of the numerous Australian figures who pass through the narrative.

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DAVID G. SCHULTENOVER. *George Tyrrell: In Search of Catholicism*. Shepherdstown, W. Va.: Patmos. 1981. Pp. xii, 504. \$32.50.

This book describes George Tyrrell's intellectual development up to and including the year 1903 when his *Lex Orandi* and *The Church and the Future* appeared. It does not treat in detail the development of his thought from 1904 to his death in 1909, the years when the "modernist" crisis in the Roman Catholic Church reached a climax and a key period in Tyrrell's life. Although not a definitive study of Tyrrell, it is a good book within its own limits. The first few chapters discuss Tyrrell from his youth to the end of the nineteenth century. The main contribution of the book, however, is its detailed description of Tyrrell's development away from a position of "mediating" liberal Catholicism to more radical positions. David G. Schultenover views Tyrrell's 1899 essays, "The Relation of Theology to Devotion" and "A Perverted Devotion," as points of departure for that development. The ensuing chapters—the meat of the book—describe in detail Tyrrell's intellectual evolution from 1900 to 1903.

Intolerance and even, on occasion, savagery have marred *some* writings and critiques in English modernist and liberal Catholic studies in the past ten years. Older writers such as Alec R. Vidler (author of *The Modernist Movement in the Roman Church* [1934] and *A Variety of Catholic Modernists* [1970]), Maisie Ward (*The Wilfrid Wards and the Transition*, 2 volumes [1934–37]), and Michael de la Bedoyere (*The Life of Baron von Hügel* [1951]) showed essentially more humane and tolerant attitudes of mind than *some* younger, narrower, and more careerist "scholars" have shown in the past decade toward points of view different from their own. Happily, Schultenover's book is virtually free of this intolerance: he is not among those who, while decrying the intolerance of the past, manifest it themselves in an opposite way in the present.

Schultenover should be commended both for producing a good book and for the extensive labor that he put into it. The following two criticisms concerning points of detail should not be taken as detracting from the work. Edmund Bishop was a lay Catholic historian, in particular, a historian of liturgy, not a "lay Catholic theologian" (p. 114). In the concluding paragraph of the book, it seems at least an exaggeration to say that anyone who has studied

both Tyrrell and the documents of the Second Vatican Council "will recognize his principles reborn on nearly every page" (p. 360).

The book is directed to a scholarly audience. It is probably too slow reading and too packed with minutiae to appeal to general readers.

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CLAIRE DOLAN. *Entre tours et clochers: Les gens d'église à Aix-en-Provence au XVI^e siècle*. (Publications du Centre d'Études de la Renaissance, number 8.) Sherbrooke, Canada: University of Sherbrooke. 1981. Pp. xiv, 433.

This book has a dual purpose: to explore intensively sixteenth-century ecclesiastical society in Aix-en-Provence and, more important, to delineate its relationships with the urban society it was supposed to serve. The book is divided into two sections, one dealing with the regular clergy in Aix and the other with the secular clergy. Claire Dolan shows to what degree the clergy integrated itself into urban life in three specific areas: economic power, civil and political power, and finally, religious influence.

After a rather cursory introduction to sixteenth-century Aix, limited by the absence of any thorough study of the subject, book 1 proceeds to a detailed investigation of the regular clergy. Here the reader is provided with an exhaustive history of the different religious orders in Aix. Their enormous wealth assured them an important position. Moreover, Dolan proves that during this period the regular clergy was the dynamic religious force, serving as both preachers and teachers. The regular clergy, however, was weakened by internal quarrels and always had to recognize the secular clergy as the true masters of this archdiocesan seat.

Book 2 deals with the secular clergy, which is portrayed as a venal and self-centered group. Although the archbishop was on the top rank of Aix society, this position was meaningless because he rarely resided there. His functions were largely taken over by the canons of the cathedral chapter. This group is examined in great detail, and the picture that emerges is one of greedy men almost totally indifferent to religious life.

What kept Aix from Protestantism was not the cathedral clergy but rather the vehement anti-Protestantism of the city's parlement, whose members played the most important role in the city's life. With the chaos of the religious wars, their power grew, and they turned Aix into a bastion for the League. Parlement's leadership arose from the clergy's inaction as well as from the fact that, traditionally, most members of the Aix chapter came from parliamentary families. A career as an Aix canon was

the epitome of social mobility for these families. In times of crisis, family alliances prevailed.

The secular clergy's real influence came from its huge economic wealth, described in considerable detail. Evoking its privileges, the chapter dispensed as little of this wealth as possible. The secular clergy was divided between the rich and powerful canons and the poor, groveling priests. The obsession for wealth alienated the lower clergy while driving the people away from institutional religion. The "culprit" was the beneficial regime that turned the secular clergy away from spiritual cares to the pursuit of wealth.

Dolan acknowledges that her local study is but a building block for further studies of sixteenth-century ecclesiastical society. Nonetheless, this book makes an important contribution to church history. The book's flaws are minor. It deals too briefly with the unique character of Provence, recently united to the kingdom and, moreover, governed ecclesiastically by the Indult, a more lax document than the Concordat. There is also scant discussion of the significance of foreign clergymen, notably Italians. Nevertheless, these faults pale beside the author's painstaking research, which is enhanced by numerous graphs, charts, and maps. Finally, Dolan's style of writing is quite readable. Although the author might have paid more attention to the broader issues of the Reformation, which would have enabled her work to transcend local history, this is a fine monograph.

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ALBERT COHEN. *Music in the French Royal Academy of Sciences: A Study in the Evolution of Musical Thought*. Princeton: Princeton University Press. 1981. Pp. xvii, 150. \$15.00.

In the seventeenth and eighteenth centuries, music was conceived of both as an art appealing through performance to the senses and as a mathematical science based on theoretical principles. It is not surprising, therefore, that music became an important area of study at the French Royal Academy of Sciences (1666–1793) and that its history mirrored the development of science during the Enlightenment. Albert Cohen's aim is to explain how the academy helped to advance musical thought and technology. The three principal sections of his book deal with the sound of music (acoustics, phonation, and audition), the craft of music (machines and inventions), and the system of music (theory and practice).

The founders of the academy had a broad, cross-disciplinary view of music. Claude Perrault, for example, wrote on the nature of sound (physics) but

also on hearing (anatomy) and on ancient music (classics). Late in the seventeenth century when the academy was reorganized and led by the Abbé Bignon and Bernard Le Bovier de Fontenelle, acoustics became the focus of its inquiries. Philippe de la Hire, an astronomer, worked on the sound of vibrating bodies; Denis Dodart, a botanist, studied vocal formation; Nicolas Malebranche, a philosopher, dealt with the relationships of light and sound. The science of acoustics culminated in the work of Joseph Sauveur, whose presentations to the academy are set forth in an appendix to this volume. Sauveur brought a rigorous experimental approach to his elaborate work on the physical basis of musical comprehension. He established the absolute frequency of pitch based on beats, and he used a logarithmic division of the octave to classify temperaments and to measure the size of intervals.

Cohen contends that the increasing emphasis in the academy on experimentation and on applied physics led science in the direction of musical technology. When scientists and craftsmen began collaborating after 1699, the publications of the academy detailed the design and construction of musical instruments. Cohen sketches the process involved in submitting and evaluating inventions ranging from chordophones and fortepianos to automatic water organs and Benjamin Franklin's glass harmonica. There is also a good account of Jean-Jacques Rousseau's system of notation.

During the eighteenth century, the academy grew increasingly interested in musical systems. Jean-Philippe Rameau's "revolutionary ideas on the nature of harmony and its relationship to the science of sound" focused the academy's discussions and stimulated scientists, theorists, and musicians throughout Europe to present their views on the essence of music (p. 82). Rameau formulated the mathematical and physical bases for a science of harmony, although the eminent composer-theorist needed D'Alembert to make his principles comprehensible to the educated public. Gradually, the theory and practice of music were welded together by the academy's insistence that musical theory must be confirmed by the aural impact of sound on the ears, minds, and emotions of listeners.

Readers of this volume may wonder about the academy's views on certain issues of the time. Did its membership disregard the *Querelle des bouffons* and the famous debate between the Gluckists and Piccinists that raged in the Parisian press for several years? Were there attempts by academy members to formulate comprehensive aesthetic systems embracing all the fine arts? To what extent did neo-Platonist ideas still permeate musical theory in the eighteenth century? I have found some evidence that a few academy members paid lip service at least to the doctrine of the effects.

Although one wishes that Cohen's discussion of individual contributions to the science of music had been more fully developed and that he had related the academy's history to the musical debates of the *philosophes* and the popularization of culture (he alludes to both in the conclusion), the author has given us a well-organized, thoroughly researched book. It is an admirable supplement to the studies of Roger Hahn, Ernest Maindron, Harcourt Brown, and Charles Paul.

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ROBERT FORSTER. *Merchants, Landlords, Magistrates: The Depont Family in Eighteenth-Century France*. Baltimore: Johns Hopkins University Press. 1980. Pp. xii, 275. \$22.50.

At the beginning of the eighteenth century the Depont family of La Rochelle were Protestant *roturier* merchants in the Atlantic sugar and slave trades. None of these attributes counted for much in the hierarchy of social qualities recognized by the *ancien régime*, and the Deponts progressively sloughed them off. Investment in ignoble areas of business and commerce were replaced by the more acceptable investment in land. Protestantism was rejected in favor of Catholicism, nobility was achieved by the purchase of office, and the family's center of gravity shifted toward Paris. In the last decade of the *ancien régime*, Deponts held royal office (one was a *trésorier de France*, another intendant of Metz), and one Depont served briefly as French minister plenipotentiary to Cologne in 1792. Despite mixed fortunes during the Revolution, the family survived with enough of its prestige and wealth intact to qualify as *notables*.

Students of eighteenth-century France will recognize this description of one well-off family's upward mobility and might even guess—correctly—at the techniques employed to achieve it: the mobilization of family capital in order to place one son well, the judicious arrangement of a marriage, the purchase of an ennobling office, the careful husbanding of capital and exploitation of inheritance laws. These avenues of social mobility have been recognized by historians, but the importance of Robert Forster's study lies in its demonstration of the process through a family biography, showing the variety of resources—"material, professional, and psychological" (p. 231)—available to the ambitious family and the considerations and circumstances that influenced the character and timing of its strategy of mobility.

It is, of course, misleading (and misrepresents the thrust of the book) to suggest that there was anything like a blueprint for mobility that was scrupu-

lously followed, generation after generation, by the Deponts. The question of social mobility here involves three broad areas: the personal, the familial, and the social (including economic and political factors). Forster has dealt in a balanced way with all three and has carefully untangled the complex relationships among them. We never lose sight of the fact that the Deponts, though members of the same family, were individuals with their own characteristics and personalities. For example, intergenerational tensions emerge from time to time over such issues as financial extravagance and neglect of religious piety (the latter, let us note, in the generally accepted context of secularization in eighteenth-century France).

But this remains a family biography, with the family treated as a corporate entity, rather than a set of biographies of family members. In analyzing the fortunes (in both the narrow and broad senses of the word) of the Deponts, Forster has struck just the right balance between his main subject—the family itself—and its social environment. He is generous in his description of the circumstances and events interacting with the Deponts, and we gain valuable insights into the workings of eighteenth-century commerce, finance, and bureaucracy, as well as provocative suggestions as to the roles of religion and Enlightenment ideas. But despite the wealth of detail and analysis that the author provides on these areas, he never loses sight of his argument or of the Deponts. Nor does he err in the other direction, committing the classic biographer's sin of making his subjects central where they were not. For example, he does not attempt to make Charles Jean-François Depont anything more than a junior member of the Committee of Thirty.

The Depont family can in no way be thought of as typical of any group in eighteenth-century France, although it does serve as an exemplar of certain social trends in that society. Forster's achievement in this family biography is to have given those trends both a broad context and human faces and to have laid bare the complicated process of social mobility with a clarity and precision that reflect the most intimate and exacting knowledge of the sources.

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FRANÇOIS FURET. *Interpreting the French Revolution*. Translated by ELBORG FORSTER. New York: Cambridge University Press or Éditions de la Maison des Sciences de l'Homme, Paris. 1981. Pp. x, 204. Cloth \$29.95, paper \$9.95.

This collection of four essays by François Furet is an important commentary on the historiography of the French Revolution. Admirably translated by Elborg

Forster from the author's *Penser la Révolution française* (1978), it consists of a lead article, "The French Revolution is Over" (1978), backed by three earlier pieces, "The Revolutionary Catechism" (Furet's well-known dispute with Albert Soboul and Claude Mazauric), "De Tocqueville and the Problem of the French Revolution," and "Augustin Cochin: The Theory of Jacobinism."

Surprisingly, Furet attributes to most historians of the Revolution, from Michelet to Lefebvre, similar methodological errors: naively accepting the revolutionary rhetoric about social antagonisms and the injustices of the Old Regime, viewing the Revolution as a rapid comprehensive overhaul, and selectively identifying within it the origins of their own convictions. Furet prescribes changed concepts, as employed by Tocqueville and Cochin, to uncover the real meaning of the Revolution. Tocqueville found continuity in the centralizing state, Cochin ideological innovation in the Terror—in both cases realities unsuspected by the revolutionists. Furet, standing on the shoulders of his two models, sees both continuity and newness in an ideology to which he attributes the revolutionary process. By defining the people "as an indistinct aggregate of individual 'right' wills . . . the revolutionary consciousness was able to reconstruct an imaginary social cohesion" in their name (p. 27). To Furet the Revolution's "message," its "lasting influence," was "democratic politics as a national ideology" (p. 26). "And," he insists, "there is no way, nor will there ever be one, to explain those notions by a social situation fraught with conflicting interests. The first task of the historiography of the French Revolution must be to rediscover the analysis of its political dimension" (p. 27). He sees the revolutionary surge from 1789 to 9 Thermidor as the activity of "groups competing for the exclusive right to embody the democratic principle" (p. 77). "In a sense, the Revolution was over" (p. 78) with Thermidor, for then "society reasserted its independence" (pp. 77–78), although the revolutionary ideology still fueled the war that was its product. Representative government, which the ideology-driven Revolution had left behind, failed again, and with the advent of Bonaparte the Revolution was really "over." France then "reconciled its two histories" (p. 79), the continuity of the administrative state and the innovation of authority embodying the people's will. But the historical importance of the Revolution remained: "it was the first experiment with democracy" (p. 79).

Readers will find in these essays a fine evocation of the revolutionary ideology and, as a by-product, much political history enriched by the perspectives of political theory. There is, however, considerable narrowness in this concentrated focus. Granted, this is a call for the revival of political history. But one wonders about the cavalier treatment of contempo-

aneous testimonies and their use by historians; about the adequacy of ideology as the motor of the revolutionary process; about the sufficiency of such an abstract appreciation of the Terror; and, finally, about the rejection of the great social interpretations, without the support of which the beautiful analysis of ideology seems a flying trapeze performance, a spectacular partial explanation of the meaning of the Revolution.

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RAYMONDE MONNIER. *Le faubourg Saint-Antoine: 1789–1815*. (Bibliothèque d'Histoire Révolutionnaire, third series, number 21.) Paris: Société des Études Robespierristes. 1981. Pp. 367.

The study of the urban, popular masses has been a major theme of French Revolutionary historiography for more than two decades. Inspired by such distinguished predecessors as Albert Soboul, Richard Cobb, George Rudé, and K. D. Tönnesson, Raymonde Monnier has made an important new contribution to this subject in *Le faubourg Saint-Antoine: 1789–1815*. It is based on meticulous research into archival and published sources and is filled with informative statistics, charts, and maps.

The average reader will doubtless be surprised by some of the data. In the popular imagination the great, tumultuous suburb is viewed as a teeming slum, whose denizens lived close-packed in sinister tenement buildings. Actually, as Monnier shows, the population was quite dense only in the oldest part, near the Bastille. Certain parts of the quarter abounded in small cultivated plots and had a rustic appearance.

Overall, however, Monnier's book reinforces and sharpens the popular image rather than obliterating it. The faubourg St.-Antoine was, above all, a working-class district. Roughly 90 percent of the male population was engaged in daily labor involving the production or distribution of glassware, wallpaper, ceramics, beer, furniture, metallic goods, clothing, and the like. Sixty percent of the inhabitants were "immigrants" from the provinces. The bulk of the workers had so little capital that they were quite vulnerable to the periodic economic crises. At any given time, one-third of the population could be classified as indigent; this number increased to more than 50 percent during the Directory.

Monnier contends that, in spite of government efforts to modernize industry and the appearance during the consulate and empire of some large textile mills, mechanization made little headway. Big establishments remained the exception. Most workers were artisans in small or middle-sized opera-

tions. Social conflict between masters and journeymen, employers and employees, was almost nonexistent. The worker lived with his "patron" and shared his ideology, a belief in small property and individual labor.

The foremost political enemies of the *sans-culottes* of the faubourg St.-Antoine were always royalists and aristocrats. But as the Revolution progressed and the artisanal community became more conscious of its dependence on merchant capitalists, a struggle developed between the "petite bourgeoisie artisanale" and the "moyenne bourgeoisie," who formed a tiny minority in the quarter. The artisanry won power in the Terror and lost it after Thermidor, but conflict persisted throughout the Directory. With the advent of Bonaparte, political and social action came to an end. The docility of the faubourg may be explained in part by Napoleon's deft policy of mixing repression of dissidents with attention to the needs of the laboring populace. The emperor took care to provide material assistance to the unemployed in times of crisis and ordered luxury items and military goods directly from the artisans, bypassing the merchant capitalists. Thus, after the fall of the empire, the citizens of the faubourg regarded it with nostalgia.

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LOUIS BERGERON. *France under Napoleon*. Translated by R. R. PALMER. Princeton: Princeton University Press. 1981. Pp. xiv, 220. Cloth \$18.75, paper \$7.95.

This is a superb translation by R. R. Palmer of Louis Bergeron's *L'épisode napoléon: Aspects intérieurs*, published in 1972 as volume 4 of the Seuil *Histoire de la France contemporaine*. It is the first synthesis of the "new history" of the Napoleonic period, which to paraphrase Albert Soboul, concentrates on *men* rather than *The Hero*. Prior to publication of Bergeron's book, the "new history" could be found mostly in French articles and a few specialized books. (The latter included works in English by Robert Forster, Richard Cobb, and J. G. Bosher; Palmer has added new ones by Emmet Kennedy, Reed Geiger *et al.*, as well as French works such as Jean Tulard's *Napoléon et la noblesse d'Empire* [1979].)

Centrally, the book deals with the development of Napoleonic elites and (inextricably connected) the evolution of the economy. It is "total history" (p. xi) in the *Annales* fashion, however, reflecting prosopographic and quantitative study and replete with lists, statistics, and maps (though no graphs). It also covers Napoleonic institutions and the mechanisms of social control, the opponents of the regime from Jacobins to draft-dodgers, demographic and even intellectual trends, and all the social classes.

The laboring poor—city and country—get short shrift, however, since, as Bergeron candidly states, information on them is lacking. (He does not say that most studies deal with the eighteenth century or earlier, the Revolution to 1794, or the nineteenth century after 1815; this needs remedying.)

Bergeron paints a picture of stagnant maritime commerce, a traditional agricultural sector, and modest, localized industrial progress. Thus, he says, France did not spawn a great number of industrial capitalists, and landed property remained the major resource (and mandatory possession) of the elite. He gives a telling sample of the notables of the Empire: a mixture of nobles of the Old Regime, new nobles, and wealthy bourgeois, but all landowners. He notes that the new nobility—one-seventh the size of the old—comprised 59 percent military men, 39 percent top-level bureaucrats or notables, 1.5 percent "talents," and only .5 percent from commerce and industry. Moreover, 22.5 percent overall were of the old nobility. Many of the latter were the largest landowners, a fact exaggerated in the original with maps that Palmer has improved with new captions.

Bergeron believes that the Napoleonic period might have been a greater turning point than it was. He sees Napoleon as an eighteenth-century rationalist who sacrificed his principles for the sake of civic order. Instead of promoting democracy, technology, and science, he based his power on property and religion (under secular control). Thus the notables, the like of whom would control France for the next century or more, stood against "the very philosophy of progress which had opened the way to their success" (p. 204).

This is a most unusual book. We should be grateful to Robert Palmer for making it available in English. It is time that Americans began seeing more history of the Napoleonic era and less history of Napoleon.

OWEN CONNELLY
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HUBERT WATELET. *Une industrialisation sans développement: Le bassin de Mons et le charbonnage du Grand-Hornu du milieu du XVIII^e au milieu du XIX^e siècle*. Foreword by PIERRE LEBRUN. (Cahiers d'Histoire de l'Université d'Ottawa, number 12.) Ottawa: University of Ottawa Press. 1981. Pp. 538. \$18.00.

Among the coal-mining regions of northwestern Europe in 1835, the Mons basin was the busiest. By 1866 that predominance had ended. The relative importance of Mons had shrunk by half, and the Ruhr region had assumed a dominance not soon overcome. Hubert Watelet has written an admirable history of both the early importance of the Mons

basin and its decline, particularly as seen through the Grand-Hornu Company, one of the region's largest in the nineteenth century.

This book is a total economic history of coal mining around Mons from the mid-eighteenth to the mid-nineteenth century. Watelet's starting point is the district's geology, and we are taken through the structure of landholding and mineral concessions under the Old Regime, the structure of transportation and opportunities for sales, and the conditions of sale imposed by governments through their systems of customs duties. These themes of geology, mineral concessions, transport, and government policy and their development during the period in question help to organize the book. When Watelet adds to them the history of entrepreneurship in the mining business and a brief discussion of the social history of the mines, the picture becomes complete. We see the peasant and artisanal mining business of the eighteenth century transformed into a mercantile business in the early nineteenth century and an industrial business by the mid-nineteenth century.

According to Watelet, the early importance of the Mons basin developed out of its favorable location, its proximity to an advantageous transport network, and its ability to provide a variety of coal that was much in demand during the early years of the Industrial Revolution, when coal was primarily used for heating. These advantages did not continue. The construction of new canals and the eventual appearance of the railroad undermined the Mons region's advantageous transportation costs. Its relative shortage of metallurgical coal closed off important markets. Finally, as Watelet's title suggests, there was a breakdown in entrepreneurship.

One wishes Watelet had made his generalizations more specific, but nonetheless it appears that he offers two explanations for the failure of development. First, in the Mons basin as a whole, he sees the premature takeover of many mining companies by bureaucratic financial organizations like the Société Générale as stifling innovation. This, in turn, can be explained by the fragmentation of many mining operations that had its origins in the land ownership structures of the Old Regime, and by the overproduction and economic crises that struck in the 1820s and 1830s. Watelet's specific example of Grand-Hornu is somewhat different. It lay outside the region of fragmentation and therefore avoided some of the problems that plagued other mines. It maintained private ownership into the twentieth century. But at Grand-Hornu there was only one great entrepreneur, Henri De Gorge. When he died, in 1832, the owners of the mine slowly followed the time-worn path into the landowning elite. They stopped striving for innovation and growth, and the company lost the competitive edge it had had before De Gorge died.

This book raises valuable questions of why firms, industries, and regions developed and then ceased to develop during the eighteenth and nineteenth centuries. It does so by considering a regional case and, within that region, an individual enterprise. Like many case studies, the book fails to draw every generalization to a logical conclusion; the reader must tease some of those generalizations out himself. But the effort is worthwhile. The case study and its context are extremely well presented, and the results are comprehensible.

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SUSANNA BARROWS. *Distorting Mirrors: Visions of the Crowd in Late Nineteenth-Century France*. (Yale Historical Publications, Miscellany, number 27.) New Haven: Yale University Press. 1981. Pp. ix, 221. \$20.00.

As late as twenty years ago, the dominant mode of writing intellectual history had a splendidly unreal quality about it: genealogies of abstract ideas intertwined to form intricate arabesques, unsullied by the mundane concerns of political, social, or economic history. True, even then the French *Annales* school was already calling for something radically different, for the historical study of *mentalités*, of the mental horizons of specific historical groups. When this call began to be heeded, traditional historians were inclined to disown the results: a notion like E. P. Thompson's "moral economy of the poor" might have its utility, but surely it had nothing to do with the history of ideas as "properly" understood. Since then, a whole new generation of intellectual historians has come into its own and Susanna Barrows's intelligent and sophisticated study measures the distance traveled. This new intellectual history does indeed deal with the "big ideas" and their intellectual ramifications, yet it situates them squarely in the social, political, and psychological context in which they are embedded.

The subject of Barrows's excellent book is the emergence in the Third French Republic of the 1880s and 1890s of a widely accepted and highly influential "discipline," namely "crowd psychology," a label that even today seems fatally linked to the name of Gustave Le Bon. Barrows is able to show that the popularity of "crowd psychology" reflected characteristic preoccupations of the era—a rash of major strikes, the Boulangist agitation, the start of May Day parades, a wave of anarchist *attentats*—that contributed to an obsessive fear of crowds. The author also seeks to show, in my view less persuasively, that the specially virulent antifeminism and the panicky concern with alcoholism that marked

late nineteenth-century France were instrumental in determining the rhetoric of "crowd psychology."

What does emerge most clearly is that "crowd psychology" was neither politically nor socially neutral. With the notable exception of Emile Zola, the founding fathers of "crowd psychology"—Taine, Sighele, Tarde, and Le Bon himself—were all die-hard reactionaries frantically opposed to social reform. Barrows is also able to provide a convincing explanation as to why and how Le Bon, a latecomer to the field, was able to turn it into his own private preserve. There is no denying his skill, both as an experienced popularizer and as a tireless public relations man for his own greater glory. No doubt it also helped to have an iron constitution, permitting Le Bon to outlast all his early competitors by nearly twenty years. Yet the author recognizes that Le Bon's triumph was more than hokum and good luck: he was unique in coming up with an appealing antidemocratic answer to the "problem" of the crowd; he alone announced the new Caesar, armed with an understanding of crowd psychology, who would, in behalf of the frightened elite, manipulate and neutralize the uncouth masses. In short, in the shadow of Le Bon looms Hitler's *Mein Kampf*.

Within the self-imposed limits of Barrows's study I can find little to criticize, though some parts of the argument strike me as more convincing than others. The book is lean, cogently organized, felicitously written, and stimulating throughout. Yet its unabashed relativism precludes raising some obvious questions. Let us grant that Le Bon and his rivals were partisan in their aims and anything but scientific in their methods; let us admit that they were haunted by bogeymen of their own day. And yet there is more to it than that, because European historians must also grant Le Bon an astounding prescience into the mass politics of the twentieth century. How *can* we explain his insight? A lucky guess? The accident that the technology of amplification—microphone and radio—ushered in an age of crowd politics? Or have we all been brainwashed to view our own epoch through Le Bon's clichés?

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MICHAEL R. MARRUS and ROBERT O. PAXTON. *Vichy France and the Jews*. New York: Basic Books, 1981. Pp. xvi, 432. \$20.95.

Under Vichy over 75,000 Jews were deported to Auschwitz. This book demolishes a number of French rationalizations as to why this happened. Michael R. Marrus and Robert O. Paxton demonstrate that Vichy's early anti-Jewish laws, rather than being the result of a German *Diktat*, were quite autonomous and that there were important links

between the "Final Solution" and the policies of the late Third Republic.

France's antisemitic tradition, weak in the early 1930s, was revived by the Depression. The Jewish "problem" was closely tied to the immigrant problem, part of the severe refugee crisis that France faced by 1938. Jews were perceived as a threat to French jobs, French culture, and peace (they might provoke a war with Hitler). The "administrative pogrom" of 1939–40 prepared the way for the callousness of Vichy, as thousands of miserable immigrants were herded into concentration camps in France no less degrading than those of prewar Germany. Liberal journalists as well as respectable professional associations called for legislation against aliens. "It is striking how widely fragments of the antisemitic position permeated moderate political vocabulary after the mid-1930s" (p. 49). Vichy built on this middle-ground consensus.

After 1940 Frenchmen who found *Kristallnacht* mobs distasteful did little to protest legal disabilities against Jews. Faced with problems of their own, especially that of food, the majority were more indifferent than hostile—although Vichy tried to make Jews the scapegoats for French ills. The left showed no more sympathy for Jews than the right: Resistance newspapers like *Combat* called for restrictions on Jewish immigration and French Jews in London tried not to "embarrass" De Gaulle by too close association. (De Gaulle himself seems never to have mentioned the plight of Jews on Radio London.)

Laval was no antisemite but simply an opportunist who hoped to curry favor with the Germans by unleashing a few doctrinaire antisemites within the government. French administrators enforced the anti-Jewish statute with a "business as usual" attitude. Professionally efficient, the French National Police developed a file in 1941 on every Jew in France that later enormously facilitated the deportations.

When brutal mass roundups of Jews began in 1942 and news of the cattle car transports began to spread, there was a public outcry (which some prefects saw as "sentimental"). It soon died down, however, when the government launched obligatory labor service for young Frenchmen in Germany; this the public viewed as much more serious, as the *real* deportation. Catholic prelates also protested at first, though less the principle of deporting Jews than the inhumane manner in which it was done. When the papacy assured Laval that it would make no trouble over the issue and when Vichy announced new state subsidies to Catholic schools and tax breaks for the church, the prelates fell silent.

In sending Jews to Auschwitz, Vichy leaders wanted to accept the German "forced labor alibi" because refugees were a drain on France's hard-

pressed budget, but the alibi did not explain why women, children, and old people were deported, why the destination was Poland and not Germany, and why the conditions of the journey itself obviously meant death for many.

Failing to acknowledge the limits of German power in time of war, Vichy did less to protect its Jewish nationals than did Romania, Hungary, Bulgaria, or even Finland, despite its later claim to have "shielded" its own Jews. Before the Germans occupied southern France in 1942, it might have emptied its internment camps and expedited (rather than, as it did, obstructed) Jewish emigration to haven countries like Spain, Portugal, and Italy. In 1943 a foreign Jew had a better chance of survival in Fascist Italy (which was openly sabotaging the Final Solution) than in Vichy France.

Marrus and Paxton have produced a brilliant study, lucidly written and keenly analytical, whose superb scholarship (based on both German and French documents) will be difficult for even the most self-serving critics to refute. It tells a chilling story of how many of France's brightest and best betrayed a vulnerable minority less out of hatred than out of expediency. Like the populace at large, they were ethical trimmers not race fanatics. Had only the latter been involved, far fewer Jews would have been taken to the cattle cars. One wishes this book would be read by many, especially by tomorrow's bureaucrats.

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JOHN HELLMAN, *Emmanuel Mounier and the New Catholic Left, 1930–1950*. Buffalo, N.Y.: University of Toronto Press. 1981. Pp. viii, 355. \$35.00.

John Hellman's critical and careful study is the latest and best work on the life and influence of Mounier (1905–50) and his journal *Esprit*, the famous avant-garde voice of the French Catholic left. Hellman also gives us a brief treatment of the role of personalism in the growth of Belgian national socialism and references to the influence of Mounier's doctrine on Prime Minister Trudeau and the pope. But Canada, Poland, and Vietnam were much less influenced than Belgium and France.

Supported by Jacques Maritain and Gabriel Marcel, Mounier became the center of a group that founded the journal and movement *Esprit* in 1932. The first aim of Mounier's pretentious program of "rebuilding the renaissance" identified his maverick position within Catholicism: disassociate the spiritual from politics, especially from the right. In 1932 Germany was regarded as the likely source of spiritual revolution, with a youthful National Socialism effecting a national revolution sorely needed in France. Strong German philosophical influences

were reinforced by contacts of the *Esprit* group with left-wing National Socialists. Maritain warned Mounier about allying personalism with a goose-stepping philosophy. At the same time religious authorities were alarmed by *Esprit's* attacks on bourgeois Christianity. Communists saw *Esprit* as another fascist, bourgeois front. Why so much interest in a small elite group publishing a journal with about fifteen hundred subscribers in 1936? A very French answer: "its originality and the interest it generated among intellectuals" (p. 73).

Mounier wanted an authoritarian revolution in France. The German invasion produced it in both Belgium and France, with the pleasant side effect of changing *Esprit* from a minor intellectual journal into a key publication in Pétain's effort to transform French youth through the Vichy *Chantiers de la Jeunesse* and the *Compagnons de France* (see W. D. Hall, *The Youth of Vichy France* [1981]). For a brief period personalists enjoyed the illusion of power, especially in the training of Vichy's elites at the *École nationale des cadres* (Uriage). Then *Action Française* and hardline collaborators forced Mounier and many Catholics out of the Vichy government. Mounier ended up in prison. It was fortunate that his double game did not work, for Mounier then had reasonable grounds for sharing in the mythology of the Resistance that became the ideology of liberated France.

The last fifth of the work deals with the ferment in postwar French Catholicism. Hellman probably gives undue space to the sophomoric Nietzschean ranting of a masochistic Mounier against the pious cowards, colorless virgins, and bags of syllogism of traditional Catholicism. But the break with Thomism was accompanied by a passionate love affair with the communists after the war, when a resurrected *Esprit* prattled on about a Marxist reformation of Christianity. Mounier had discovered that illusion of alienated believers, the humanistic Marx. Disillusionment with Stalinism led Mounier to seek intellectual salvation in existentialism, which stressed the personalist virtue of commitment. Secular philosophies mattered little in the end: the mystical body of Christ was the true source of the anti-individualistic and anticapitalistic spirituality of personalism.

Some of the best minds, or at least pens, in France were associated with *Esprit*—for example, P. H. Simon, Hubert Beuve-Méry, Jacques Ellul, Henri Marrou, Merleau-Ponty, Paul Ricoeur. Hellman's book admirably shows us how a mediocre and tedious philosopher with administrative and educational genius was able to harness some of their energies in attempting to reconcile Christianity and revolution.

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FRANCIS O'NEILL. *The French Radical Party and European Integration*. New York: St. Martin's Press. 1981. Pp. ix, 124. \$19.95.

The Radical party of the Fourth French Republic was a pale reflection of the group that had played a dominant role in France's politics during the inter-war years. When the National Assembly turned in 1950–54 to the divisive votes on whether to share national sovereignty with such novel bodies as the European Coal and Steel Community (ECSC) or the European Defense Community (EDC), the Radical party had only 68 deputies. At the height of its popularity, in 1932, it had 160. Nevertheless, the party retained influence quite disproportionate to its number of deputies and to its percentage of the electoral vote, which after 1945 never exceeded 8 percent. A Radical was premier in ten of the twenty-two governments of the Fourth Republic, and Radicals held important ministries in every government.

The Radical party was a party of leaders without followers, and, as strong-willed personalities in a party that imposed little discipline, its members were quite prepared to express disagreements with each other in parliamentary votes. The most notable split occurred over the EDC treaty, which was supported in 1954 by 33 Radicals and opposed by 34. Furthermore, even though the party had voted almost unanimously in favor of ECSC in 1951, it split again in 1957 over the vote on the Treaty of Rome creating the European Economic Community, with 36 in favor and 19 against. Nor were these divided votes the result of a calm agreement to disagree. René Mayer's bitterness against Pierre Mendès-France for allowing the EDC to go down to defeat was a principal motive in his attack seven months later that provoked the fall of the Mendès-France government.

Francis O'Neill is well aware of the difficulty of writing a history of the Radical party's attitude to European integration, since the party did not have a unified approach to the problems of supranationalism. His solution is to analyze the evolution of the viewpoint of individuals within the party, as evidenced in their parliamentary speeches and, in a few instances, in interviews and in private papers to which he gained access, such as those of Mayer and Pierre de Félice. He makes little attempt to discern how groups within the party formulated concepts of foreign policy, except to document the Europeanism of the group around Mayer and Maurice Faure and the nationalism of Édouard Herriot and the *radicaux classiques*. It is doubtful whether a more extended analysis would have done more than illustrate the fragmentation of view, however, even within the party's constituent "tendencies." For example, O'Neill finds massive exceptions when he tries to show that the Europeanists were similar in

youth, governmental experience, and industrial background. Senior Radicals like Henri Queuille led the integrationists; the Mendèsistes who opposed supranationalism were mostly young.

As a succinct narrative of the evolution of the attitudes of individual Radical party members toward the principal measures of integration between 1949 and 1957, O'Neill's book is helpful. But the sketches remain insubstantial, and the broader influences on the Radical politicians, especially the development of the French economy, are largely unexplored.

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IRIS H. W. ENGSTRAND. *Spanish Scientists in the New World: The Eighteenth-Century Expeditions*. Seattle: University of Washington Press. 1981. Pp. xiv, 220. \$25.00.

Spain's diverse American possessions provided a valuable laboratory for the study of nature that was so much a part of the eighteenth-century Enlightenment. It is therefore ironic that few Spaniards, peninsular or American, are included among the lists of giants, from Alexander von Humboldt to Charles Darwin, who led major expeditions into South and Central America. In this study of Spanish scientific expeditions, Iris H. W. Engstrand suggests that Spain's lack of prominence in natural history was not the fault of government disinterest or lack of training and dedication on the part of the scientists themselves.

During the second half of the eighteenth century, a circle of enlightened ministers around the king actively supported the efforts of progressive scientists to expand scientific knowledge and to build up the institution so essential for research. This was no mere academic exercise; rather, science promised, and the crown expected, concrete payoffs. The "useful knowledge" produced by scientific knowledge would lead inevitably to economic growth and improved living conditions. Indeed, as Engstrand implies, it was the failure of natural history to live up to its promise of immediate results that cut short the support its followers needed.

This monograph is a detailed narrative of two major expeditions assigned the task of observing and classifying the flora and fauna of Spanish North America. Initiated and led by Martín de Sessé y Lacasta, a physician with extensive experience in the army and in America, and Alejandro Malaspina, a naval officer with world-wide experience, respectively, and endowed by the crown, these small expeditions embarked on superhuman tasks. Between 1785 and 1796, these hardy bands of natural-

ists, at times split into smaller groups, explored thoroughly central and southern Mexico from Acapulco to Aguascalientes, Guatemala, Baja California, and the Pacific Northwest as far north as Alaska. Everywhere they collected thousands of specimens, produced drawings of animal life (many are included here), and noted the customs of the Indians they encountered. Sessé and his colleagues also established the Royal Botanical Garden of Mexico, where they planned public lectures and classes and collected samples for its collection of plants.

Engstrand's meticulous research in a variety of scientific archives allows her to tell us the story of each expedition in minute detail. We discover the background of each participant, precise lists of the equipment carried, and thorough analyses of the reports and samples sent back to Spain. Like scholars skilled in grantsmanship today, these eighteenth-century naturalists had to be as adept in handling politicians and in acquiring and budgeting funds as in engaging in actual research. Unfortunately, although the narrative tells us much about the work of such expeditions, it is less helpful in analyzing the scientific significance of their findings. To be sure, such criticism may be unfair, since the monograph is in fact the story of the achievers, not the long-term significance of their results.

Like their colleagues throughout Europe, Spanish naturalists were convinced that the accumulation and codification of knowledge would in itself produce results. They devoted limited time and resources to collecting as many samples as possible, the more exotic the better. In characteristic Enlightenment fashion, a predilection for the "natural" extended to people: the Malaspina group praised the more primitive Indians of the Northwest, while emphasizing the "major vices [of] laziness and making love" (p. 83) of Chilpancingo Indians. Nevertheless, as Engstrand correctly points out, these eighteenth-century observers differed from their sixteenth-century counterparts in their conviction that their discoveries would eventually lead to man's understanding of nature's laws.

This fine monograph confirms what we know of the successes and failures of science under the Bourbon monarchs. Spanish scientists were up to date in their methods and knowledge; they were familiar with the latest works of their colleagues abroad. Many had military or medical backgrounds; most were employed by royal institutes outside the traditional university establishment; all depended on the royal treasury for support. Their shortcomings were due not to second-rate science but to political intrigue, financial difficulties, and excessively optimistic expectations on the part of the crown, as the demise of both expedition leaders illustrates. Sessé overspent his budget and allotted time; when he finally returned to Spain, six years

overdue, he was ignored. Malaspina was imprisoned as a result of his foolish involvement in a dispute between Manuel de Godoy and the queen.

The only weakness in the volume lies in its acceptance of the exuberance of the late eighteenth century itself. The scientists of the Enlightenment were not as unique as they believed. Belief in laws of nature was not new but different; contact with foreigners and royal patronage for learning was not new but expanded and redirected. The author's uncritical dismissal of earlier Spanish achievement does not detract from the value of the narrative, however.

The volume includes several useful appendixes, helpful maps, and many attractive reproductions of drawings from the expeditions. It is a valuable addition to our understanding of Spanish science and to the nature of research in the eighteenth century.

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DANIEL R. HEADRICK. *Ejército y política en España, 1866-1898*. Translated by JAIME MELGAR BOTASIS. (Serie de Historia.) Madrid: Tecnos. 1981. Pp. 293.

In this study Daniel R. Headrick addresses the problem of military intervention in the political life of nineteenth-century Spain. The title of the book is somewhat misleading, for its contents are almost evenly divided between the first and the second half of the century. Only the revolutionary period 1868-74, which Headrick labels "the most important event in the history of Spain between the war of Independence and the civil war" (p. 211), receives detailed scrutiny.

Admirably, Headrick provides a comparative perspective for his study of Spanish militarism. He concludes that the Spanish case more nearly resembles the domestically oriented praetorianism of the Latin American military than the externally directed militarism of the great European powers. In both Spain and Latin America, the collapse of the Old Regime created a political vacuum that could not be filled by a weak and divided middle class, who therefore turned to the army for political leadership and protection from the rebellious masses.

In Spain, the institutionalization of praetorianism was essentially complete by the 1840s. But the Revolution of 1868 upset the alliance between the crown, the civilian elites, and the military hierarchy. In the chaotic six years that followed, the army was badly divided by the absence of strong leaders and by the aggressive antimilitarism of the federal republicans, who came to power in 1873. When General Arsenio Martínez Campos restored the Bourbon monarchy in 1874, both civilians and

officers were eager for a period of stability and order. The Restoration settlement of Antonio Cánovas del Castillo achieved this by providing a political system that could operate without military tutelage and by supporting the military reforms that least divided the army.

Headrick argues, correctly, that it is not possible to understand military intervention without taking into account the institutional characteristics that encouraged the army to act. His analysis of the professional issues that lay behind the *pronunciamiento* politics of the first half of the century is more successful, however, than his discussion of civil-military relations during the Restoration, when the army abstained from overt intervention. Additional research in the published primary sources—especially the *Colección legislativa de Guerra* and the Cortes debates—would have strengthened his conclusions in this section.

In an attempt at prosopography, Headrick studied 186 generals out of approximately eight hundred to one thousand who reached that rank between 1868 and 1898. Because he restricted his sample to those for whom he was able to locate published biographical data, it was necessarily skewed in favor of the highest ranking and most illustrious officers. More interesting from the point of view of an institutional study, however, are the six hundred to eight hundred generals who did not achieve fame or biographical notice. Headrick's sample obscures the gulf between the minority of ambitious professionals and politicians and the majority of bureaucratic officers, whose conflicting aims and interests made it difficult for weak civilian governments to formulate coherent military policy. To determine the career patterns of average officers, Headrick might have consulted the personnel records in the Archivo General Militar in Segovia. A recent study of the social origins of the nineteenth-century officer corps by Fernando Fernández Bastarreche (*El ejército español en el siglo XIX* [1978]) shows what can be done in this regard.

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OLE DEGN. *Rig og fattig i Ribe: Økonomiske og sociale forhold i Ribe-samfundet, 1560–1660* [Rich and Poor in Ribe: Economic and Social Conditions in the Society of Ribe, 1560–1660]. In two volumes. Summary in English. (Jysk Selskab for Historie, number 39.) Aarhus: Universitetsforlaget. 1981. Pp. 458, 402.

This exhaustive two-volume study of Ribe's economy and society during the century following 1560 is local history on a grand scale. Ole Degn has been

inspired by the approaches toward social and economic history taken in Börje Hanssen's *Österlen*, which was published in 1952 and deals with south-eastern Scania; Pierre Goubert's *Beauvais* (1960), which studies that province of northern France; and A. M. van der Woude's *Het Noorderkwartier* (1972), which deals with the western Netherlands north of Amsterdam. "But in contrast to these works," Degn writes (vol. 1, p. 21), "the investigation here goes a step further, for consideration in the present work of the physical condition, population conditions, and ways of making a living only serve as a starting point for discussion of the social and economic conditions seen interdependently, and the income, living conditions, and other ways of life of different groups of the population."

Space allows only a brief summary of this very detailed work. In general its focus is on the individual or individual groups. Economic, social, and political differences are emphasized, but interrelationships between individuals and groups are not neglected. In the first of four parts, entitled "The Population and the Physical Environment," both Ribe and its surrounding district in southwestern Denmark are included in the physical description, and the relative size of various groups within the population is estimated in percentages. Part 2, "Means of Support," includes a discussion of labor conditions in general and much more specifically of those in agriculture and fishing, trade, shipping, crafts, day labor, domestic service, and civil service. Part 3, the heart of the work, discusses the economic and social differences between various groups, including contrasts in personal finances, housing and family conditions, standards of living, and intellectual life. This extended discussion is followed by separate chapters dealing with privileges for the rich and lack of them for the poor, social status and mobility, and social unrest. The last part deals with economic, social, and demographic developments during the period under study, together with the causes behind these changes. Drawing together some of the results obtained earlier, the conclusion discusses the importance of the relatively low level of production and the rigid barriers between social groups. The crisis of seventeenth-century society is explained as being due to a combination of problems within capitalism itself and those brought about by the demands of central government.

As implied by even this brief summary, Degn's study is based on a large amount of archival material in Ribe and elsewhere, both in Denmark and abroad. As the author points out, this material provides in a number of ways unique access to Ribe's economic and social history during the period 1560–1660. The quite varied information from the archives is summarized in a number of tables; thus if Degn's conclusions are challenged, at least the data

from which he draws them are elucidated. There are, in fact, 116 tables included in the main text in volume 1 and 95 more, usually longer, in volume 2. In addition, there are many judiciously chosen illustrations and sixteen maps.

In short, *Rig og fattig i Ribe* is a most welcome addition to the field of urban history, providing new or underestimated interpretations and material at almost every turn of the page. More than that, it provides a model of how best to write local history. These advantages are not restricted to readers of Danish. Degn includes a summary in English (vol. 2, pp. 16–34).

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LEENA KAUKIAINEN. *Småstater i världskrisens skugga: Säkerhetsfrågan i den offentliga debatten i Sverige, Finland och Danmark, oktober 1937–november 1938* [Small States in the Shadow of World Crisis: The Public Debate on the Security Question in Sweden, Finland, and Denmark, October 1937–November 1938]. Translated from Finnish into Swedish by EVA STENIUS. (Historiallisia Tutkimuksia, number 112.) Helsinki: Suomen Historiallinen Seura. 1980. Pp. 261.

Leena Kaukiainen is a Finnish historian who has published a study in Swedish on the small states in the shadow of world crisis, concentrating her attention on the public debates on security problems in Sweden, Finland, and Denmark from October 1937 to November 1938. This was a critical period in European history. The small Northern and Eastern European states had to reckon with the rise of Nazi Germany and its growing appetite on the one hand and the ever-present danger emanating from the Soviet Union on the other. Strangely the author does not include in her study either Norway, the westernmost Scandinavian country, or the southern neighbors of Finland—Estonia, Latvia, and Lithuania—although they faced similar problems. These countries are mentioned cursorily, almost by accident. Therefore Kaukiainen's book loses some appeal.

The author limited her research to the study of documents found in the archives of her native Finland, and in Sweden, Germany, and England, but not Denmark. She has, however, gone through the pages of the most important Finnish, Swedish, and Danish newspapers and parliamentary debates, and the extent of consulted books, brochures, and magazine articles is quite impressive. Books on the Baltic states and some major books of general nature are missing, however.

Although the author does not emphasize it, there was no unity among the Scandinavian and Baltic

nations. Norway was oriented toward the west, Denmark worried about its vulnerable southern borders with Germany, Finland was overshadowed by its Russian neighbor, and Sweden tried to stay as far away as possible from too close relations with more endangered Finns and Danes. All of them tried to exclude the Baltic states as the most endangered area. All of them were united in the vain hope that absolute neutrality would protect them better than entry into one of the major blocs of great powers. They decided to stay away from their obligations toward the League of Nations, and each one of them hoped that in case of clear and present danger they might suffer less than their neighbors. They had no courage to build their own Scandinavian, Finno-Scandinavian, or Scandinavian-Baltic military bloc of common interests. Because of socialistic and neutralistic inclinations in the past, the Scandinavian countries had also disarmed themselves and had thus made themselves easy targets for predatory powers. No ringing declarations of absolute neutrality and no false pretense of their exclusive security could protect them. During World War II only Sweden managed to bungle through the ever-present dangers. One cannot hide sympathy, however, for the peace-loving Scandinavian, Finnish, and Baltic nations who could not hide their dislike for atrocities committed by totalitarian great powers but who did not want to get involved and harm anybody.

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HERMANN KELLENBENZ. *Deutsche Wirtschaftsgeschichte. Volume 2, Vom Ausgang des 18. Jahrhunderts bis zum Ende des Zweiten Weltkriegs.* (Beck'sche Sonderausgaben.) Munich: C. H. Beck. 1981. Pp. 544. DM 45.

Like its companion volume 1, Hermann Kellenbenz's *Deutsche Wirtschaftsgeschichte*, volume 2, traverses the economic history of Germany with authority and ease. Focusing on the period between the late eighteenth century and the end of World War II, the book provides a clear, concise, and reliable guide to the industrialization of Germany, its people, processes, and events. It would be difficult to find a work that accomplishes more efficiently and effectively what Kellenbenz sets out to do.

Industrialization is, as we all know by now, as much a sociocultural and political as an economic and technological affair. It demands, therefore, an analysis steeped in the interdisciplinary study of the preconditions and precipitants as well as processes and results of this most complex instance of modern social change. Kellenbenz succeeds admirably in this assignment, owing in large part to a methodology

that is as logical and straightforward as it is wide-ranging. To begin with, he divides his work into three major segments, determined by changes in the characteristics of German industrialization: the period from the French Revolution to the founding of the German empire, from 1870 to World War I, and from 1914 to the collapse in 1945. Then the author examines each period systematically, by the same scheme he applied in volume I. First, he establishes the parameters of German industrialization—its intellectual-ideological, scientific-technical, social-demographic, and political-governmental milieus. Second, he explores its variables as they were affected by, and in turn affected, the industrialization process—that is, the agricultural, manufacturing, and service sectors—and their roles in the business cycle. Finally, he traces the outcome of Germany's industrialization via its impact on prices, wages, public finance, and gross national product and income distribution.

Thus the book provides, as its author planned, a useful survey and reference work for the layman or beginning student of economic history. Clear graphics and simple maps as well as a deliberate deemphasis on economic theory and jargon contribute to this result. Three separate registers ease the reader's access to the book's abundant details, and an extensive (but mostly German-language) bibliography suggests further avenues of research. An epilogue summarizes the author's conclusions about the role of politics, ecology, protoindustrialization, and Rostow's "takeoff" in Germany's modernization. These are all topics very much at the heart of the current debate over the nature and definition of the industrial "revolution" and, like the book itself, deserve a wide and thoughtful audience.

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CHRISTOPH PRIGNITZ. *Vaterlandsliebe und Freiheit: Deutscher Patriotismus von 1750 bis 1850*. Wiesbaden: Franz Steiner. 1981. Pp. 221. DM 44.

The purpose of Christoph Prignitz's book is to describe the development of German patriotic thinking from the mid-eighteenth century to the German revolutions of 1848–49. In particular, the author traces the history of the bourgeois-libertarian (*bürgerlich-freiheitliche*) content of patriotic thought, since "modern" patriotism in Germany arose during the Enlightenment largely as the creation of German middle-class intellectuals desirous of an economic, political, and spiritual freedom—a kind of general emancipation from the constraints of a still-feudalized society.

Before 1789 the patriotic ideal, philanthropic and

broadly cosmopolitan, had relatively little national or political content and aimed at the amelioration of human society in general, with little in the way of concrete proposals for reform. Only with the outbreak of the French Revolution were German intellectuals presented with a politically concrete model of patriotism, which many of them could embrace precisely because the French ideals and achievements were initially seen as serving the cosmopolitan cause of humanity at large. Even the German Jacobins, Prignitz submits, whose advocacy of popular sovereignty placed them in a radical position not shared by the more moderate liberal intellectuals, cannot be regarded as "unpatriotic" but as a group that simply gave to already widely accepted enlightened ideals a specific political formulation.

Disillusionment among the German intelligentsia with the degenerated later republic and with Napoleon's subsequent despotism began on the one hand to depoliticize patriotic thinking once again, while on the other hand it created a more markedly nationally oriented patriotism: the earlier universalistic goals remained but were now increasingly couched in terms of a specifically German mission to the world. At the same time, German romanticism, emphasizing the individuality of peoples and the need to look to the past for the lineaments of the genuine *Volksgeist*, powerfully reinforced the German national ideal but also established a new definition of freedom—accommodation of the individual to the organic historical experience of his kind—that was sharply different from the rationalism and progressivism of the Enlightenment.

The tendency of romanticism to denigrate the rational and democratic legacies of the great Revolution helped turn Germany away from Western Europe in the realm of political ideology—a development made still more pronounced by the aggressively anti-French spirit of the liberation era. Patriotism now took on an increasingly exclusive aspect, while the sense of a special German mission to Europe became much stronger. But, Prignitz points out, although the humanitarian-universalist generosity of earlier patriotism was narrowed somewhat, the libertarian ideal remained strong among the majority of national-liberal thinkers until 1848. Prignitz finds, however, the growth of a real antagonism between nationalism and libertarian patriotism by the 1840s. After the defeat of liberalism in 1848–49, this conflict led to the increasing co-optation of nationalist ideals by conservative forces, which not only finally stripped the patriotic vision of its libertarian content but also, after 1870–71, led Germany toward chauvinism and the bellicose integral nationalism of the Wilhelmine era, with all its fateful consequences.

Although Prignitz's findings do not add very much that is genuinely new to the insights of a

number of historians who have written on the development of nationalism, patriotism, and liberalism in Germany, his book is still a welcome contribution by virtue of its tightly organized approach to an important complex of ideas that has not elsewhere been dealt with so exclusively and systematically.

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HANS BRANIG. *Fürst Wittgenstein: Ein preussischer Staatsmann der Restaurationszeit*. (Veröffentlichungen aus den Archiven Preussischer Kulturbesitz, number 17.) Cologne: Böhlau. 1981. Pp. ix, 260. DM 64.

For the era of reform and the period of the restoration in Prussia, few figures have been portrayed in such consistently negative terms by contemporaries and historians as Prince Wilhelm Ludwig Wittgenstein (zu Sayn-Wittgenstein-Hohenstein). This judgment is unlikely to change as a result of this study, despite the probable intentions of the author, Hans Branig.

Nevertheless, this is a solid, straightforward work that adds to our knowledge of Prussian politics in the first half of the nineteenth century. It is based on extensive archival research, primarily in the large Wittgenstein *Nachlass* deposited in the *Geheimes Staatsarchiv Preussischer Kulturbesitz* in Berlin-Dahlem. Before retirement, Branig was senior archivist at the archives, edited the Hardenberg-Wittgenstein correspondence (published in this same series), and acquired mastery over the very difficult handwriting of the prince—no small achievement. The study traces Wittgenstein's career in a simple, chronological manner. He entered Prussian service in 1792, was soon appointed steward to the queen, and by 1810 was lord high chamberlain to Frederick William III. This established his close relationship with the royal household of Prussia, his base of power and influence for more than thirty years. This position was strengthened in 1819, when he was appointed minister for the royal household, a title he held until his death in 1851. He was also used periodically for a variety of minor diplomatic missions, usually involving the affairs of the princely families of Germany. His most visible work came when he served as minister of police during the period surrounding the negotiation and enactment of the Carlsbad decrees.

Throughout all these years, Wittgenstein was a reactionary. He defended the status quo tenaciously and was vigorously opposed to virtually every innovation proposed or implemented in Prussia. He was an opponent of Stein, carried on numerous intrigues behind the back of Hardenberg, and was instrumental in the forced resignation of Bernstorff in 1832. He worked assiduously to block the intro-

duction of a constitution in Prussia in 1819, was highly critical of the state's reformed military, advocated the harshest possible censorship, and objected to the creation of the *Zollverein*. By 1818 he was slavishly devoted to Metternich and virtually adopted, and passed to the king, the Austrian's arguments on matters facing the Prussian state.

Most of this is discussed in considerable detail by Branig, but he consistently plays down the destructive influence of Wittgenstein's activities. Moreover, he examines Wittgenstein within an unusually narrow analytic framework. Part of this may have been determined by the fact that Branig did not have access to the governmental archives and *Nachlässe* at Merseburg and therefore did not have the documentation required for a broader treatment. Still, many questions remain unanswered, and there are numerous omissions. The author never analyzes in any depth either Frederick William III, the source of Wittgenstein's power, or his relationship with Wittgenstein. The discussion of the prince's opposition to reform from 1806 to 1819 is curiously cut off from any integrated analysis of what was really at issue in the reform era. The mention of Wittgenstein's opposition to the *Zollverein* and to Prussia's tentative move toward a less reactionary approach to domestic, German, and European politics from 1828 to 1832 is only slightly related to the broader range of issues in question during this period. Moreover, there is no systematic discussion of Wittgenstein's political philosophy—formal or informal, relating it to the major political events he lived through. Finally, the subordinate relationship of Wittgenstein (a mediatized prince) to Metternich is nowhere explored satisfactorily. Thus Branig's book is in many ways disappointing because the opportunity for writing a broader, more analytic study, profiting from the author's unrivaled knowledge of the Wittgenstein collection, has been missed. Nevertheless, historians of Germany will be grateful for a work that clearly sets forth the basic elements of Wittgenstein's political career and expands significantly our knowledge of this important conservative figure of the early nineteenth century in Prussia.

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DAVID BLACKBOURN and GEOFF ELEY. *Mythen deutscher Geschichtsschreibung: Die gescheiterte bürgerliche Revolution von 1848*. Translated by ULLA HASELSTEIN. (Ullstein-Materialien, number 35068; Sozialgeschichtliche Bibliothek.) Frankfurt a./M.: Ullstein. 1980. Pp. 139. DM 12.80.

Recently two young British historians have stolen into the entrenched camp of German historiogra-

phy and planted a small cluster of explosive charges. The resulting detonations still reverberate across the land, especially in the general vicinity of Bielefeld, where the new university has in the past decade become a kind of Vatican City of the so-called "Kehrite" or "critical" school. What began in the 1960s as a protest movement against the ideological and methodological conservatism of an older generation has now become, in its turn, a new orthodoxy with its reigning pontiff (Hans-Ulrich Wehler), its own propaganda organ (*Geschichte und Gesellschaft*), and its cadre of fervent clerics (nearly everyone from Ralf Dahrendorf to Fritz Fischer). So, at least, claim the intrepid authors of the two muckraking articles published here in fluid German translation.

The iconoclastic style is quite deliberate and sometimes strident. David Blackburn and Geoff Eley set out to expose the foibles and faults of the current conventional wisdom in Germany, and they spare few reputations in doing so. Nothing personal, mind you. With a certain amused irreverence they peer at their Teutonic protagonists across the English Channel and across yet another generation gap. Many of their observations and criticisms, though not always original, are highly persuasive and should well serve the intended purpose of requiring some careful rethinking. Nonetheless, since they are so eager to set the record straight about almost everything, they occasionally distort in their own fashion the image they aim to correct. The caricature of a caricature does not always provide an accurate sketch.

Even the most rapid summary of this slender but tightly packed paperback volume must distinguish between the different tones of its authors. Eley is the more aggressive. He charges that Wehler and his cohort have gullibly accepted the dubious notion of a unique course (*Sonderweg*) of German history in consequence of the failure of a bourgeois revolution in Central Europe in 1848 and thereafter. Thus, according to that version, a preindustrial elite maintained control of the state, resisted modernity, stunted democracy, manipulated the middle class, and finally afforded fertile soil for the roots of fascism. Eley interprets this scenario as a specific German variation of a more general modernization theory, which he heatedly rejects. He characterizes the "Kehrite" doctrine variously as idealistic (because the bourgeoisie is defined by a common mentality), reductionist, normative, and unhistorical. Heading this long parade of errors, Eley finds, is a simplistic conception of English history, which the Germans have adopted as a model but which is in fact a mirage. The best of recent British scholarship, he contends, does not sustain the hypothesis of a bourgeois revolution in England either. Thus the implicit ideal of German historians derives from a superficial reading of England that is secondhand,

dated, and confused. Eley describes the basis of Wehler's work as a Manichaean teleology: on the one hand a newly polished Whig interpretation of Britain and on the other the doomed and allegedly unique course of German history. Both, according to Eley, are fictions. A closer look would reveal that the two nations in reality shared a similar experience as they adapted to the advance of the industrial process. The German stress on uniqueness is vastly overdone, and hence the *Sonderweg* leads only to a dead end. Instead, Eley maintains, Germany needs to be considered among Western polities as a variant, not a deviant.

Many of Eley's caveats cannot be easily gainsaid. Still, he has a tendency, which he finds deplorable in others, to overinterpret and oversimplify. The historians he castigates are surely not so benighted nor their views so fatuous as he pretends. The political incoherence of nineteenth-century German liberalism is, after all, not a figment of some fevered historical imagination. And the British experience does present some striking and demonstrable differences from that of Germany. Beyond that, the tenor of Eley's critique is somewhat intemperate. He is notably indiscriminating in closely juxtaposing German scholars whose academic origins, intellectual concerns, and historiographical methods are very diverse. He exaggerates their conformity to a single creed. Worse, he tends to treat them like a gang of naughty schoolboys who are obliged to pull down their britches for a brief thrashing by the master. One wonders whether his own petulance altogether suits Eley for such a self-assigned role.

By contrast, Blackburn's prose seems more relaxed and measured. The brunt of his argument is that German historians have erred of late by over-emphasizing what did *not* happen (the bourgeois revolution) rather than concentrating on what actually occurred. He is led to repeat many of Eley's misgivings in a less obsessive manner: the definition of the bourgeoisie is unduly intellectual, the contrast with England is distorted, the insistence on Germany's uniqueness is overwrought, and so forth. Blackburn dismisses the *Sonderweg* thesis with the lapidary observation that every case is naturally in some sense unique. And he, too, underscores the similarities among industrializing societies, all of whom have experienced the phenomenon of embourgeoisement. To be sure, aristocrats continued to exercise influence in Germany—as in England—but they were also obliged to conform to the changing circumstances effected by industry. Contrary to the claim of the "Kehrites," the decisive factor was not the manipulation of preindustrial elites but the character of the German bourgeoisie, which increasingly set the norms of style, dress, and comportment. Blackburn even goes so far as to speak of the "hegemony of the bourgeoisie" (p. 98).

At the same time, he does not neglect to confront the weakest point of his argument: in politics the German bourgeoisie was manifestly unsuccessful in creating a constitutional monarchy. Yet, Blackbourn insists, that failure was only a matter of comparative degree, and we should accordingly evaluate the German case as an integral part of the total political constellation of Western Europe. The bourgeoisie was not merely thwarted from above but was the victim of its own disunity, complexity, and self-contradiction. The lack of a unified political reform movement in the midst of rapid social change should therefore not be surprising. The Kaiserreich deserves to be evaluated in its own terms and not simply as a prelude to the German catastrophe.

Blackbourn thereby seeks to shift the emphasis from continuity to *conjunction*—not to deny the importance of historical continuity but (in his favorite image) to examine the other side of the coin. There is of course a distinctly Rankean thrust in the proposition that every epoch is *unmittelbar zu Gott* and that the historian's primary task should be to discover *wie es eigentlich gewesen*, with the laudable difference of a new emphasis on social history. Blackbourn makes a strong case for adopting such a stance. But he is notably better at asking questions than at answering them. Most of his suggestions for a historiographical reorientation remain elusive. This fact cannot be explained solely on the grounds that his essay is necessarily brief. He has the irritating habit, for example, whenever making reference to England, of throwing in France for good measure, even though he cites not a single French source! If a serious argument for the relative homogeneity of the West in the late nineteenth century is to be made, as Eley and Blackbourn intend, such glaring imprecision will need to be eliminated in order to remove certain assumptions from the realm of pure conjecture.

Despite some obvious shortcomings, then, these two essays provide a fresh and stimulating look at the state of the historian's craft in Germany. They perform the important service of placing elemental postulates and their corollaries into question. Above all, they offer a compelling case for comparative history. One cannot read this volume without concluding that it is past time for historians of modern Europe to transcend the narrow confines of national history.

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L. L. FARRAR, JR. *Arrogance and Anxiety: The Ambivalence of German Power, 1848–1914*. (University of Iowa Studies in History.) Iowa City: University of Iowa Press. 1981. Pp. xiv, 221. \$15.00.

This short volume, which puzzlingly constitutes the inaugural volume of the "University of Iowa Studies in History" series, purports to show that the First World War did not proceed from caprice, stupidity, or megalomania in the leadership of Europe prior to 1914 but instead was the inevitable result of a system characterized by the enormous amassment of "power" by the great nation-states tied to one another by various alliances. Neither diplomatic skill nor statesmanship, no matter how astute, could prevent the inexorable denouement of power in war. L. L. Farrar, Jr. naively quantifies the power that he invests with such an exclusively deterministic force as a combination of industrial production and population. We are thus informed, for example, that in 1871 Britain had 34.7 percent of Europe's power, whereas France could claim only 20.2 percent. Military expenditures are also fed into this calculation, with an equally precise, and equally absurd, air of statistical finality. The only alternative to power is decline, and, according to Farrar, a state that fails to make periodic use of its might will lose first its credibility and next its independence. Given the choice, war is the preferable option and preventive war its most prudent variation. Germany's enormous power instilled the nation with arrogance, but at the same time anxiety developed, since circumstances for a war favorable to Berlin seemed to be declining. German behavior was thus an odious combination of assertiveness and quavering, an amalgam Farrar somehow imagines that he has discovered.

The European system of great, powerfully armed, and intricately allied states undoubtedly contributed to the outbreak of the war, inasmuch as it created vast military machines ready to prove their fighting mettle, inflated with *amour-propre* a file of rulers and statesmen in no way equal to the vast authority they possessed, and limited the diplomatic freedom of action available to individual states in moments of crisis. The author's monomania with "power" and its inexorable consequences, however, leads him mistakenly to eliminate or neglect the subtle factors that need to be elucidated in order to understand how 1914 happened. He claims that diplomats and statesmen were powerless to affect the course of events determined by power, but in fact he notes that the way the past was perceived (and by whom other than these very officials?) often contributed to the policies adopted on the eve of the war. Without some grasp of the slender abilities and alarming irresponsibility of the epigones of the post-1890 era, it is impossible to comprehend not only the tortured diplomacy but also the deep-seated collective neurosis that gripped Europe before 1914 and that was a significant ingredient in conditioning the powers for war. Economic considerations, which in some cases worked to improve but in others to

darken international relations, likewise receive no acknowledgement of importance. Domestic perplexities that weighed heavily in the minds of statesmen in Berlin and elsewhere are lightly and unconvincingly dismissed as mere reflections of power itself. Yet Farrar points to a rightist revival in Germany just before the war that was very much the creation of forceful reactionary personalities. This resurgence was not an attribute of some astringent abstraction of "power" but instead proceeded from a strongly held conviction that traditional privileges, public order, and the fatherland's preservation were gravely threatened from both within and without.

When Farrar can liberate himself from his obsession with "power" he is capable of producing a clear narrative of prewar diplomacy. But what the captive reader more often will encounter are pages of contrived paradox and a really stupefying indulgence in alliteration. Those who are disturbed by the notion on page 31 that in a certain situation "prestige, protection, pique . . . and public opinion were probably contributing factors . . ." will undoubtedly be badly shaken by a relationship that is pictured seventy-six exasperating pages later as "coincidental, casual, complementary, congruent, or complex." The capacious assertion by Laurence Lafore in the foreword that this is a work of Platonic subtlety displaying a "huge and ramified substructure of erudition" and "monumental learning" cannot be seriously entertained. This spongy argument is based exclusively on familiar printed sources that are not impressive in their extent and certainly do not constitute sufficient ballast for the extravagant argument for which they are marshaled. For the author, or anyone else, to understand why Berlin entered into the course it did in the decades before 1914 will require a deeper burrowing into the printed record, a long exile in the archives, and, most importantly, a broader and more flexible perspective on the past than is to be found here.

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MANFRED HANK. *Kanzler ohne Amt: Fürst Bismarck nach seiner Entlassung, 1890–1898*. Munich: Tuduv, 1980. Pp. ii, 505.

This book by Manfred Hank is the second edition of a larger version in photographed typescript—some eight hundred pages—published in 1977, the same year in which Wolfgang Stribrny's *Bismarck und die deutsche Politik nach seiner Entlassung, 1890–1898* appeared. The present version has been set in type, bound in hard covers, and enhanced with well-selected illustrations, but it has lost not only all the source notes and appendixes but also all scholarly references in the text, including a good part of the

introduction and all of the summary conclusion. Consequently this edition is of little scholarly use, which is unfortunate, since the book is probably the most substantial study of the subject to date. More than one hundred pages longer than Stribrny's work, even without notes, it simply contains more information—a sensitive account of Bismarck's final illness and death, for example.

Based on a thorough familiarity with and exploitation of the Bismarck Archive in Friedrichsruh as well as material from the papers of Philipp zu Eulenburg, Bernhard von Bülow, Hermann Hofmann, and others, the book is organized as a topical analysis, with accompanying advantages and disadvantages. Useful and informative sections, for example, are devoted to the chief operators of Bismarck's propaganda machine, from the relatively independent Maximilian Harden of *Die Zukunft* and Hugo Jacobi of the *Münchener Allgemeine Zeitung* and the *Berliner Neueste Nachrichten* to the more slavish, harshly treated and exploited Moritz Busch, Horst Kohl, Hermann Hofmann of the *Hamburger Nachrichten*, Johannes Penzler, and Paul Liman. The topical approach, however, leads inevitably to some repetition and a loss of narrative coherence and dramatic force. Especially with the omission of the conclusion, things do not get tied together.

Essentially Hank agrees with Stribrny that Bismarck's "unremitting opposition" to everything done in Berlin after his fall from power in 1890 was motivated by patriotism rather than mere rage or hatred. He knew just how precarious conditions were both at home and abroad. Hank goes further than Stribrny, however, not only in pointing out Bismarck's real feelings about William II—and all his oppositional activity was ultimately aimed at the kaiser—but also in claiming that the old man would have done more had he been able, that his strange ambivalence, the lassitude and hopeless resignation that alternated with energetic actions, stemmed from a keen realization that his ever-growing, motley following were not a serious, dedicated, and cohesive political force. His followers loved his criticism, and acclaiming his heroic figure gave them a legitimate (and safe) way to demonstrate against the kaiser, but they had no intention of doing anything to challenge or change the system. Hank points out that the old man's most loyal lieutenants, Kardorff, Krupp, and Henckel von Donnersmarck, were careful not to go so far as to cut themselves off completely from the court. In addition to the habit of authoritarian subservience, the brash overconfidence of the era also undermined Bismarck's oppositional movement—a subservience and a confidence that he had helped create.

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MARGARET LAVINIA ANDERSON. *Windthorst: A Political Biography*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xi, 522. \$69.00.

From the moment he emerged as undisputed leader of the Catholic Center party in the early 1870s until his death in 1891 Ludwig Windthorst was recognized as imperial Germany's outstanding parliamentarian and Otto von Bismarck's most resourceful and dedicated political opponent. In this reassessment of his career and his political achievement—the first full-length biography of any merit published since 1907—Margaret Lavinia Anderson evaluates the nature and formation of Windthorst's political thought, describes his parliamentary skills and accomplishments, and appraises his contribution to Germany's constitutional development.

Although Anderson has much to say about Windthorst's person and career, what lies at the heart of this biography is the argument that under Windthorst's leadership the Center "pursued the most consistently liberal policies of any party in both the Prussian and the imperial parliaments" (p. 8). According to Anderson, Windthorst and his coreligionists were driven in the direction of comprehensive political reform because they themselves were the victims of Bismarck's repressive policies during the bitter church-state conflict known as the *Kulturkampf*. In contrast to those familiar interpretations that emphasize political Catholicism's opportunism and its willingness to sacrifice constitutional principles for the satisfaction of special interests, Anderson contends that to solve the problems faced by the Catholic church and its adherents in Bismarck's Germany Windthorst worked to "deabsolutize the State" (p. 281). In pursuit of these aims, she argues, Windthorst endorsed the democratic franchise, resisted attacks on parliament's budgetary rights, and above all fought for the preservation of private and public freedoms against exceptional legislation.

Yet for all Windthorst's dogged resistance to Bismarck's authoritarianism, Anderson acknowledges, the Catholic political leader remained frightened of popular sovereignty and hesitated to commit himself to parliamentary rule. Given the permanent numerical inferiority of Catholics in imperial Germany, the Center could never aspire to a parliamentary majority, and Windthorst was thus unwilling to create opportunities for other parties like the anticlerical Liberals or Free Conservatives to exercise political power. For that reason, admits Anderson, Windthorst did not seek to gain power for himself or his party, because he was afraid parliamentary government could also be used to oppress his coreligionists.

If the liberal label for the Center party and Windthorst's policies must be qualified in so fundamental a fashion by Anderson herself, one is tempt-

ed to ask if liberalism is the right concept at all. To characterize Windthorst's goals as liberal is to obscure the hybrid and ambiguous character of his policies. Windthorst's object, the evidence suggests, was to abrogate what he believed were unpopular laws and to prevent the encroachment of the Bismarckian state on the rights of the Catholic population and other religious and ethnic minorities. Even where this aim required reforms typical of the traditional liberal program, Windthorst's goal was not to advance the cause of liberalism but to obstruct Bismarck. To emphasize the former over the latter is to confuse methods with aims, effect with intent.

Despite Anderson's difficulties with the contradictions inherent in Windthorst's aims and interests, her book represents a significant contribution to the literature regarding the political development of Bismarck's Germany. It not only provides a useful and well-documented description of parliamentary politics and procedures, but—because of its discussion of the *Kulturkampf* and the relationship of Windthorst and his party to both the German episcopate and the Holy See—it also draws attention to the importance of religion in German politics and reminds us that the challenge to Bismarck was not confined to liberals and socialists.

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WINFRIED BECKER. *Georg von Hertling, 1843–1919. Volume 1, Jugend und Selbstfindung zwischen Romantik und Kulturkampf*. (Veröffentlichungen der Kommission für Zeitgeschichte, series B, Forschungen, number 31.) Mainz: Matthias-Grünewald. 1981. Pp. xxxviii, 361. DM 64.

Georg von Hertling is probably best remembered as the elderly German chancellor who resigned in 1918 rather than preside over the parliamentarization of the imperial government. He was hardly a member of the Wilhelmine establishment, however, and his appointment as chancellor had been evidence of its imminent collapse. High office in the Second Reich would not normally have been bestowed on an undistinguished philosophy professor, a Center party politician and parliamentary delegation leader, or the prime minister of Bavaria's first parliamentary government. This well-documented study of Hertling's long career is welcome and overdue.

Winfried Becker's first volume takes its subject to the age of thirty-three, when he was an apparent failure in the academic world and was just entering politics. He had been born in Darmstadt, in the Catholic "diaspora" where a few close-knit families formed a small minority among the Protestant

population. His father's family were of the minor office-holding aristocracy in Hesse; his mother's family were cultivated Frankfurt bourgeois, and his maternal grandmother was a member of the well-known and talented Brentano family. Throughout his formative years Hertling received material and psychological support from his extensive family connections. In 1867, after an education that included study at universities in Münster, Munich, and Berlin, he completed his *Habilitation* in philosophy at the university in Bonn, overcoming his distaste for living in Prussia, the destroyer of his hopes for a Greater Germany under Austrian leadership. Both Catholic and Protestant faculty members at Bonn were shortly to be roused to strong emotions by the proclamation of the dogma of papal infallibility by the Vatican Council. Although the vast majority of German Catholics accepted the dogma, many university professors and theologians did not, and they were upheld by the Prussian government. Hertling's cousin, the theologian Franz Brentano, left the priesthood rather than accept the dogma, and several other friends and colleagues joined the Old Catholic secession.

Hertling, after initial doubts, not only joined the minority camp in the faculty that accepted the dogma but also published articles and made speeches actively defending it, thereby losing friends and potential sponsors for a future professorship. The Prussian Ministry for Education favored only "free-thinking Catholics" (p. 179) for vacancies, even at Catholic institutions. The Bonn faculty refused to support his application for an extraordinary (unpaid) professorship but permitted him to continue teaching because his lectures were heavily attended by seminary students. Professional ostracism led Hertling to join the Catholic resistance movement. He joined the Center party, and in 1875 his gifts of rhetoric won him a seat in the Reichstag. His success as a politician eventually opened the door to a professorial chair at the University of Munich.

The biography's lengthy descriptions of Hertling's neoscholastic philosophical writings will probably be of little interest to historians or, indeed, to philosophers. Yet they are relevant to the author's conclusions about Hertling's early years. In philosophy, religion, and politics he was consistent in rejecting the dominant liberal, evolutionist, and mechanistic outlook of his age. Perhaps a second volume will reveal how the stubborn young dissident developed into the cautious, tired chancellor of 1918.

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RICHARD BLANKE. *Prussian Poland in the German Empire, 1871–1900*. (East European Monographs,

number 86.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. xii, 268. \$20.00.

Poles comprised one of every ten Prussian subjects and were the largest ethnic minority in the German empire. Yet the history of Prussian Poland, in spite of its tragic significance for the twentieth century, has been neglected in English because of the formidable linguistic skills required for closer study. Richard Blanke's book makes good a serious deficit. Based on work in Polish and West German archives and containing excellent maps and appendixes, it is broader than the dates in the title imply and notable for giving equal attention to both sides of the Polish-German relationship.

The decades on which Blanke focuses were an important turning point. Ironically, just as Prussian Polish leaders were giving up their revolutionary aspirations, Prussian leadership abandoned its traditional toleration and embarked on a repressive course: the *Kulturkampf*; the expulsion of thirty-two thousand Poles and Jews who could not prove Prussian citizenship; and legislation exploiting the economic vulnerability of the Polish gentry, reeling under falling agricultural prices, by buying up their estates (a complicated subject that Blanke analyzes with clarity). School policy was a particular focus of reforming zeal. Polish teachers were transferred to German areas, German replaced Polish as the language of instruction, Polish was eliminated as a subject and private instruction in Polish was prohibited, Polish students became ineligible for state scholarships, and funds voted to improve education in the East were reserved for already superior German schools. Poles might well conclude that their government intended them to be illiterate in their own language and permanently consigned to menial positions. Blanke explains these dramatic changes in policy on a number of levels; not least important were the views of Otto von Bismarck, which were closer to those of his nationalist countrymen than earlier generations of historians believed (although Blanke's revisionism is more tentative than Helmut Neubach's *Die Ausweisung von Polen und Juden aus Preussen, 1885/6* [1967]). The response to these measures was reflected in the upsurge in Polish self-help organizations, the discrediting of traditional Polish leadership, and the development of a nationalism that mirrored the most cynical strains of its German counterpart and spread to hitherto unawakened Kashubia and Upper Silesia.

The action in this book is mostly at the level of high and middle politics. One is curious about the grass roots. How did German teachers feel about facing uncomprehending pupils? What was the "war situation" [p. 230] between Germans and Poles in Posen? A final careful reading might have

eliminated a few obscurities and inevitable errors in detail (Adalbert Falk's replacement in 1879 was Robert von Puttkamer, not Friedrich von Eulenburg, who was never *Kultusminister*; Wilhelm Emmanuel von Ketteler died in 1877 and could not have declined the Gniezno-Poznan see in the eighties; the 1881 Reichstag elections gave the opposition a majority of more than one hundred, so the eighteen-member Polish delegation did not hold the balance of power). Scholars will regret that page references to parliamentary speeches are never cited and that this important book was not given a better index.

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PETER PARET. *The Berlin Secession: Modernism and Its Enemies in Imperial Germany*. Cambridge: Belknap Press of Harvard University Press. 1980. Pp. 269. \$20.00.

Peter Paret's study of the Berlin Secession is a welcome addition to the growing body of work linking in a single field of cultural analysis the concerns of the modern political historian with those of the art historian. The subtitle of his work, *Modernism and its Enemies in Imperial Germany*, accurately reflects the author's agonistic posture: he is chronicling a struggle, that of the pioneers of a new art with the politically entrenched defenders of artistic orthodoxy. What lends special interest to the tale is the skill and originality with which Paret analyzes its institutional dimensions.

Perhaps nowhere does the anachronistic character of the German empire emerge more strikingly than in state policy toward the arts. In reading Paret's fascinating descriptions of the structure and control of artistic education, production, marketing, and exhibiting under William II, one almost feels transported back to the era of Louis XIV, with the monarch at the center of the organization of the arts, so that their ideology and practice could serve to discipline the minds and inspire the hearts of the subjects in the interest of the crown.

Nowhere did William II make his "personal regime" more strongly felt than in his role as Supreme Art Lord. He immersed himself deeply in the affairs of the academy, the Institute of Fine Arts, and the state museums; he followed closely the art shows for their content, attendance, and sales; he intervened in the awarding of prizes. The result was a powerful reinforcement of nineteenth-century traditional academic art just at the time when the first winds of modernism began to blow into Germany from the West. William II was nothing if not explicit about his sociopolitical aims: "We Germans," the emperor declared, "have permanently acquired [the] great ideals [of classical art], while other peoples have

more or less lost them . . . [I]t is part of these ideals to enable the working and toiling classes, too, to become inspired by the beautiful and to help them liberate themselves from constraint of their ordinary thoughts and attitudes."

Paret shows that the idealism in art espoused by the emperor and held by his best academic painters and bureaucrats were basically consistent with popular middle-class taste. Official art policy set its face resolutely against both social realism and impressionism. The social realists were seen as subversive for their glorification of domestic misery, the impressionists as alien for their introduction of the French virus of ethical neutrality and decadence into the healthy idealistic culture of German art.

Against this complex of ideas and institutions, Paret shows, modernists had to begin their quest for freedom as an economic struggle. They had to break the primacy of the state-controlled salons as the art market through which painting was certified and displayed for sale. It is the special merit of Paret's account of the Secession to show the enormous role played by the art dealers, Paul and Bruno Cassirer, in the organization of the artists for their aesthetic breakthrough and economic independence. "L'art pour l'art" and "Business is business," those two great autonomistic principles that proved such powerful solvents of unified European culture in the nineteenth century, appeared in the context of the official Prussian art scene in vigorous alliance. From papers of the Cassirer family and firms, the author shows how the dealers lent their administrative and financial skills to the new artists in organizing themselves into free associations and mounting the independent exhibitions that created a public and a market for their work. In this parapolitical conflict, the Goliath that was the Supreme Art Lord, with his hosts of academic painters, met his match in the David of Paul Cassirer, the art entrepreneur.

The climax of this struggle was reached over the selection of the German art to be displayed at the St. Louis Exposition of 1904. Here questions of German prestige and the demands of the international art trade threw the royal intervention on behalf of an increasingly outworn art into ridiculous relief, choking off the possibilities of drawing in the New World to redress the aesthetic balance of the Old. The result was that the excluded modern artists and their allies organized on a national basis in Weimar a *Künstlerbund* "to protect and further individualism" in art. And that principle, the spokesman Count Harry Kessler added, "will also have to be accepted in the political life of the nation." The parallels that Paret suggests between the St. Louis affair and the *Daily Telegraph* affair give focus to his narrative. Yet in the end the individualism that the new liberalism in art espoused was no stronger than it proved in politics. The Secession's pure aestheticism, however liberating, could not withstand the rise of radical

expressionism within or the clarion call to war without. In the light of this fate, Paret sees the Secession movement as "the aesthetic expression of doomed liberalism in Germany."

As an analysis of the nature and failure of imperial German art policy, *The Berlin Secession* is a splendid contribution. It also clarifies in a new way the institutional history of emergent modern art in Berlin. The intellectual history of that art, its ideational content as revealed in its forms, remains to be analyzed. Paret has scarcely tried to penetrate the meanings of the painting whose creators' struggle for independence he traces. His distance from such problems is best indicated by the enormous gap between his illustrations and his text. The pictures are seldom addressed in the text, and some of the more crucial discussions of what painters are doing pass unillustrated. Instead, he presents pictures from his own collections—enviable from a connoisseur's point of view, but of only the most minimal relevance to the problem in hand. Scholars more interested than Paret in penetrating Secessionist art itself for its historical content will have their work cut out for them. But thanks to Paret's work, they will be able to undertake it in a clearly defined political and institutional framework.

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BARBARA GREVEN-ASCHOFF. *Die bürgerliche Frauenbewegung in Deutschland, 1894–1933*. (Kritische Studien zur Geschichtswissenschaft, number 46.) Göttingen: Vandenhoeck und Ruprecht. 1981. Pp. 313. DM 58.

After years of neglect, serious scholarly attention now is given the historic women's movements of Germany. This research truly is enriching, offering new and challenging perspectives on a host of complicated issues in German history. The findings not only uncover a surprising activism of women but also shed light on areas of considerable debate, including the country's ambiguous liberal tradition as well as the appeal of fascism in the 1920s. Barbara Greven-Aschoff's work is a welcome addition to the growing literature on the German *middle-class* women's movement—specifically those multifarious groups that worked independently of existing political parties and attracted members who came from an essentially bourgeois social milieu. It follows precisely the chronological division of Richard J. Evans's earlier study (*The Feminist Movement in Germany, 1894–1933* [1976]) and shares with him the notion that bourgeois feminism stemmed from political liberalism, despite evidence that links its origins to religious reform movements in Vormärz. But this is a very ambitious and innovative study. It uses the German movement as a test case to work

out a sociological interpretation of the emancipation movements that began in the nineteenth century and to determine the significance of the move for women's equality in the general process of social change. Readers will be delighted that the author confronts directly problematic feminist issues such as the impact of labor and wages on familial power relations. And the overall perspective is intriguing because Greven-Aschoff begins with the recognition of the *partial* success of the movement for women's liberation up to today.

The book starts with a description of the emergence and spread of norms and values that helped determine women's place and self-image in nineteenth-century Germany. Following is a lengthy and complex discussion of notions of public and private, reasons for the idealization of women's role in the family, and the contribution of Rousseau, Fichte, and the Romantics to hardening assumptions about women's essence (*Wesen*). The author persuasively shows that women activists found, absorbed, and challenged these new norms as they formed their own movements beginning in the second half of the nineteenth century. This novel approach, however, is not sustained throughout the book, and its remaining two-thirds offers a rather straightforward account of women's interest-group politics over four decades. Here Greven-Aschoff distinguishes three broad types of women's organizations: professional and educational associations, those oriented toward welfare and social-reform activities, and—the heart of the women's movement—general organizations pursuing specifically emancipatory goals. Yet only the relations between the first and third are developed, and in analyzing the latter the author adopts the traditional dualism of "radicals" and "moderates" and subscribes to the idea of a short-lived radicalization in the early twentieth century. Specific discussion also includes suffrage, wartime welfare activity and representative politics in Weimar, and the difficulty of surviving in the increasingly unfavorable political climate of the late republic. The conclusion is a good one and draws the material together. But divisions within groups are not clearly delineated; often only one woman's writings are used to characterize a whole wing, and the validity of the choice never is established. Greven-Aschoff also does not clearly distinguish her assumptions from those of the historical subjects. Nonetheless, I came away excited by many new ideas, such as the functional use of "neutrality" to bridge differences within the broad movement and the ingenious appropriation of notions of "women's essence" to prevent males from taking over female teaching jobs.

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DAVID ABRAHAM. *The Collapse of the Weimar Republic: Political Economy and Crisis*. Princeton: Princeton University Press. 1981. Pp. 366. Cloth \$30.00, paper \$12.50.

This study in political economy (the subtitle is exact) is in many ways a distinguished book. The analysis is both complex and remarkably clear. It represents a most ambitious attempt at a critical synthesis of a huge secondary literature, and yet the argument is sustained throughout by effectively chosen quotations from a wide variety of primary sources. Experts will surely disagree about the merits and weaknesses of specific components of David Abraham's interpretation, but his study unquestionably has the virtue of being useful: it constitutes a large focal point for further argument and research, and, unlike many recent monographs in the field, most of it can be read by undergraduates. The author presents *The Collapse of the Weimar Republic* as the necessary (but not sufficient) condition for the Nazi seizure of power. He has little to say about the rise of Nazism. The prior and for him more important question is: why were the economically dominant classes and groups in German society unable to agree with each other about how Germany should be governed after 1924?; how did this "inability of fragmented dominant groups to organize and unify their interests" lead to a situation in which Hitler's chancellorship appeared to be the only option?

The framework of the analysis owes a lot to Antonio Gramsci and Eckart Kehr. Abraham makes it serve to give shape and accent to a detailed and yet coherent story of the shifts in the alignments within the political and economic power structure of the Weimar Republic. He rightly places the conflict between capital and labor in the center of the stage, but he gives special emphasis to identifiable *potential* alignments that, in the event, failed to coalesce as durable hegemonic systems of power, and to the reasons why they were stillborn or short-lived. His discussion of relations between the reformist labor movement and the agrarian interests, and of the constraints within which the principal industrial fractions attempted to evolve their agricultural policies, are particularly imaginative and acute examples of this mode of writing. Abraham's overall argument concerning why by the winter of 1932-33 heavy industry and the Prussian latifundia still had basic interests in common; why these interests predominated politically over those of industrial exporters, dairy farmers, and trade unions; and why the former interests required a Hitler government is too sophisticated to summarize. It is more appropriate to the bold and critical spirit in which the book is written to point here to areas of contention and to loose ends.

First, contrary to other recent studies, Abraham

argues that the axis between the trade unions and the "dynamic" capital-intensive sector of industry was powerful and effective in the late 1920s and that the reformism of the Social Democratic labor movement was both "militant" and successful to a degree that really did make this axis (reinforced as it was by state power) a serious threat to the viability of heavy industry. The latter's massive counteroffensive from late 1928 on was, on this reading, not a willful exercise in authoritarian union-bashing but belonged firmly in the realm of economic necessity: the coal, iron, and steel industries did have their backs to the wall. Abraham makes a good case, although he may overestimate the material gains made by workers in regular employment as a result of the constellation of forces that held the ring until 1930. Abraham calls this constellation "the tacit Silverberg-Stresemann-Severing alliance." This type of nomenclature is essential to his mode of analysis. It necessarily leaves the problem of historical agency ill defined. A tacit alliance is a different kind of historical actor from a capital shortage, both are different from a coalition government, and an agreement between the SPD and the DNVP on the desirability of organizing the grain trade is something else again. Proposals that "sought, or tended, to split peasants from estate owners" (my italics) at one level must, but at another cannot, be conflated. In grappling with this problem, to which there simply are no clear solutions, Abraham is usually careful in his choice of terms, but he does not discuss the problem explicitly at sufficient length. The difficulties are especially apparent in the concluding pages. Although these difficulties are irreducibly theoretical and cannot be resolved into a set of component empirical problems, Abraham's account of the breakdown of the power blocs does have one salient empirical flaw: the German army is almost completely missing from the changing patterns of determinant alignments. Even though Michael Geyer's pioneering studies appeared too late for the author to take account of them, the omission is odd, for the incorporation of the role of the military would probably not burst the conceptual framework of this study in political economy.

Statistics constitute one essential form for the representation of economic forces in history. Abraham's handling of statistical evidence seems a little insecure. Although the problem of real wage levels (see above) may be a matter of interpretation or judgment, there are several other points, especially in the sections on agriculture, where the figures themselves are unclear and the inferences drawn from them uncertain. Further, although some of the new calculations that Abraham has done with the industrial statistics are enlightening, these figures need to be correlated with each other in every possible way in order to establish the truly signifi-

cant trends and links. The fact, for example, that a very few branches of industry had a massive preponderance in Germany's export trade after 1924, does not of itself prove the existence of a homogeneous export sector of industry. We need to know the (changing) proportions of output and profit that the relevant major producers directed into, and derived from, the export trade. This small issue of method raises larger questions of substance if, as the book positively invites us to, we extend Abraham's story forward beyond 1933. His findings imply very strongly that the enormous flexibility of the major exporting concerns of the 1920s in adapting to and furthering Nazi policies of autarky was the central fact in developments between 1933 and 1936. This flexibility had origins in the 1920s that are not explored here ("fractions" within IG Farben . . .). Such a query is something more than a quibble, but it must be emphasized that this book is immensely suggestive for the study of the political economy of the Third Reich. The terms of the debate about the "primacy of politics," for instance, now need to be, and can be, comprehensively redefined.

Abraham dedicates his book to his parents, "who at Auschwitz and elsewhere suffered the worst consequences of what I can merely write about." His critical distance from this terrible source of inspiration never fails him. As a result, he has made a major indirect contribution to our understanding of Nazi rule.

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MARTIN HÖLLEN. *Heinrich Wienken, der "unpolitische" Kirchenpolitiker: Eine Biographie aus drei Epochen des deutschen Katholizismus*. (Veröffentlichungen der Kommission für Zeitgeschichte, series B, Forschungen, number 33.) Mainz: Matthias-Grünewald. 1981. Pp. xxvii, 160. DM 46.

This excellent study of one of the most controversial figures in the recent history of the Catholic church in Germany was originally accepted as a doctoral dissertation at the Free University in Berlin. In addition to research in numerous church and state archives and wide use of published documents and monographs, Martin Höllen conducted correspondence and interviews with some seventy persons who were involved in the matters discussed here. This latter information helps considerably in filling out the author's account and constitutes valuable source material for future researchers. That the author includes in his voluminous notes hard-to-find biographical data on virtually every person mentioned is worthy of special note.

Heinrich Wienken (1883–1961) was the son of

peasants in Oldenburg who were persuaded to permit their son to study for the priesthood. He proved to be an excellent scholar. At the seminary in Innsbruck he roomed with Polish theological students who, along with later contacts with Russian émigrés and Polish workers in Germany, turned Wienken into a lifelong Slavophile. Ordained a priest in 1909, he was sent to Berlin. Here his organizing ability and interest in social-welfare problems led to an appointment in 1916 as representative of the Catholic League of Charities in Berlin. He became deeply involved in post-World War I relief activities, especially youth work, and he organized the first projects for sending city children to the country in the summers. His work in the 1920s brought him into close touch with his superior, Cardinal Bertram of Breslau, and the papal nuncio Pacelli, the future Pius XII. Wienken was placed in charge of channeling papal famine relief to Russia. He came to know many governmental officials as well as his counterparts among evangelical and socialist welfare leaders. He served on the board of film censors and from 1929–33 was a member of the Berlin city parliament. But he was not politically minded; his interest first and last lay with helping the poor and needy. When the Nazis came to power in 1933 he continued to carry on his work much as he had heretofore. He thought that the sharp edges of the Nazi movement could be smoothed and that Nazism might even be baptized (pp. 35, 49).

When the Fulda Bishops' Conference decided in 1937 to establish a commissariat in Berlin as a liaison office between the church hierarchy and the government, party, and Gestapo, Wienken was the bishops' choice to head the office. The pope had just appointed him coadjutor bishop of Meissen. Not only was Wienken able and experienced, but he also had the strong backing of important members of the hierarchy (Bertram, the head of the bishops' conference; Gröber of Freiburg; Berning of Osnabrück) and of the papal nuncio Orsenigo.

Wienken was not an initiator of policy but rather one who carried out directives. He consulted regularly with Cardinal Bertram, who held (ca. 1940) that there were two wings in the Nazi party, a radical antichurch and anti-Christian one, and another inclined to conciliation. It was wise policy to attempt to strengthen the latter (pp. 49, 94). This involved negotiation rather than confrontation, a policy that often led to a rather humiliating role for Wienken. Bishop Preysing of Berlin, almost alone among the German bishops, was at odds with Cardinal Bertram and advocated more forthright statements and public denunciation of Nazi policies. Aside from an account of Wienken's role in the conflict over euthanasia, the author does not go into detail about his activities on behalf of prisoners, especially priests, in

the concentration camps; the Christian Catholic Jews; the foreign laborers; or the many restrictions placed on the Catholic church. That he spoke up and at times obtained concessions, there can be no doubt. Whether more aggressive action on his part, as his critics maintain, would have produced greater results, lies at the center of all controversy over Wienken. But in his attitudes and political practices, as the author points out, Wienken was in line with the great majority of German bishops.

After the war Wienken continued to negotiate with the Russians as well as with the other Allied authorities in Berlin and eventually with the East German authorities. He was able to bring about some changes in the constitution of the DDR in favor of the churches. As he had been throughout his life, he was chiefly concerned with helping the needy. To this end he was willing to negotiate with whatever government was in charge. As he stated when in 1951 he took over the bishopric of Meissen: "I will not be lacking in good will to work with [the government]. For I am deeply convinced that the wellbeing of the people (*Volk*) is best served when the state and church work together peacefully and in harmony with each other" (p. 144). In summary, this is a fine, information-packed monograph.

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RAINER C. BAUM. *The Holocaust and the German Elite: Genocide and National Suicide in Germany, 1871–1945*. Totowa, N.J.: Rowman and Littlefield or Croom Helm, London. 1981. Pp. ix, 374. \$25.00.

Much recent research has focused on the role of traditional German elites in the Final Solution. This is a question of great importance, for without elite acquiescence (and at times active cooperation) the genocide of the Jews would not have been possible. In this work Rainer C. Baum, a sociologist at the University of Pittsburgh, attempts to account for the moral indifference of the German elite, a deficiency that he argues was uniquely developed. For Baum moral indifference in the face of mass murder had little to do with negative attitudes toward Jews. Moral indifference, he argues, long predates the Holocaust and is best conceived as a continuing functional response of Germany's elites to the anomalies of modern German history and social structure.

Because they never developed a common system of values (Baum terms this "value dissensus"), Germans remained "ethical strangers" to one another. Politics reflected this fragmented condition and were based on the rule of interest constellations each pursuing its own ends. This led to the "destratification" of elites characterized by their one-dimensional character ("Germany developed pecu-

liar elites with command over but one of the socially recognized scarce resources rather than of several" [p. 68]). Value dissensus tended to make morality a private matter and to encourage a conception of the public domain as essentially amoral. Destratification created insecurity. One-dimensional elites formed a "cartel of anxiety": relative status-deprivation deprived them of the basic tools necessary for the assumption of moral responsibility. Under these conditions the very notion of morality in public life appeared hopelessly romantic and irrelevant. Moral indifference became anchored in German political reality.

Baum is concerned with an important problem, but his analysis is flawed in many respects. Despite his disclaimers, the work is insufficiently historical and lapses into reductive sociology: Nazism and the drive to empire in both world wars are reduced to problems of elite status-deprivation and their attempted resolution through the externalization of power politics. Comparative analysis with, say, British or French elites prior to World War II would have demonstrated that imperial habits and treatment of subject populations differed in no significant way from the German example. German elites were quite capable of, and concerned with, the proper treatment of "ethical strangers." Even under the brutalized conditions of World War I, a relatively small percentage (5.4 percent) of Russian prisoners of war died in captivity. In World War II the figure was up to 57.8 percent. If moral indifference was a constant, then other intervening variables become critical to an explanation of Nazi atrocities. Baum never considers the historical backdrop that made it easier to rationalize the killing of Jews. His omission of antisemitism is perhaps his most serious failure. As a result of this he is never able to get to the crux of the purported subject of the book: the uniqueness of the *Endlösung* (p. 1). Antisemitism was not a sufficient condition for the Holocaust, but it certainly was a necessary one. Because of his dismissive attitude toward the role of beliefs, stereotypes, and ideology, Baum totally evades this factor; he also does not take seriously the Nazi *Weltanschauung*. But precisely these are the central questions raised by the Holocaust: What was the distinctive dynamic of the Nazi movement and why did it choose the Jews—rather than other "ethical strangers"—as the central target destined for nothing less than total extermination? The Jews were not random victims, and, although indifference permits murder, it should not be confused with it.

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HERMANN WIESFLECKER. *Kaiser Maximilian I.: Das Reich, Österreich und Europa an der Wende zur Neuzeit*. Volume 4, *Gründung des habsburgischen Weltreiches*,

Lebensabend und Tod, 1508–1519. Vienna: Verlag für Geschichte und Politik. 1981. Pp. xxviii, 691. S 870.

In this meticulously researched and extensively footnoted fourth volume of his projected five-volume biography of Emperor Maximilian I, Hermann Wiesflecker examines the political and military enterprises of "the last knight" during the final decade of his life. Wiesflecker effectively details the emperor's attempts to improve the administration of the empire and of his Austrian patrimony, his efforts to assure the Habsburg succession in Spain, his struggles to terminate the "eternal war" over the Gelderland, his laying down of the foundation for the later Danubian monarchy of the Habsburgs by negotiating the Treaty of Pressburg of 1515, and his tentative resolution of difficulties with the Polish king, Sigismund, over the allegiance of the Teutonic Knights.

The focal point of Wiesflecker's study, however, is Maximilian's attempt to reestablish imperial hegemony over Italy. When the emperor-elect turned once again toward Rome in 1508 and undertook what he hoped would be a triumphal march through Italy that would culminate in his own coronation as emperor by Pope Julius II and in the effective reassertion of imperial rights and prerogatives in Italy, he was confronted by the determined opposition of the Venetians and by a papacy reluctant to encourage an increase in Habsburg power and prestige in Europe. Thus began the "great European war for Italy" that sapped the energies and resources of the emperor-elect and his Austrian patrimonial lands.

Maximilian's persistent and often frustrating military and diplomatic campaigns (including his musings about being crowned pope) have often been characterized as foolish, treacherous, and haphazard. Wiesflecker argues, however, that they were entirely consistent with Maximilian's ideal goal of establishing a universal Christian empire. Up to the very day of his death on January 12, 1519, Maximilian exhibited an unswerving devotion to the idea of the *translatio imperii* to the German nation and his own house of Habsburg. Moreover, while his Italian campaigns failed miserably, Maximilian's policies and efforts elsewhere were far from dismal failures. It is true that his enterprises were doomed in Italy and restricted elsewhere by a number of factors: the obdurate refusal of the German estates and, finally, of his own patrimonial lands to provide him with sufficient money and troops or to substantially improve the emperor's agencies of governance; the tenacious opposition of the Venetians; the duplicity of the papacy; the lukewarm support of his sometime allies, the French, Spanish, and English monarchs; and even the reluctance of his heir Charles to support him fully. Nevertheless, Maximilian's efforts did culminate in the founding of a Habsburg

"universal" monarchy under Charles V, albeit based upon the wealth and power of the lands ruled from Madrid and Vienna rather than from Germany.

Wiesflecker's monumental study, although redundant at times, is impressive, and his argument, aptly summarized in the final chapter, is persuasive. Despite the pacific implications of the old humanist doggerel "Others wage war; you, O blessed Austria, marry," for Maximilian war was as essential an agency of attaining his goals as diplomacy. Nevertheless, Maximilian was in many respects his own worst enemy. By relentlessly pursuing his idealistic and grandiose conception of a universal empire, he exhausted the resources of his patrimonial lands, devastated northern Italy, and so alienated the estates of his own lands that he was unable to gain their consent to effect necessary fundamental reforms in the administration of the empire. His dream of establishing a universal Christian empire proved as incomplete and empty as its only lasting concrete manifestation, his monumental tomb in Innsbruck.

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HUBERT CH. EHALT. *Ausdrucksformen absolutistischer Herrschaft: Der Wiener Hof im 17. und 18. Jahrhundert*. (Sozial- und Wirtschaftshistorische Studien, number 14.) Vienna: Verlag für Geschichte und Politik. 1980. Pp. 256. S 296.

This book more than fulfills the promise of its main title. Relying heavily, though not obtrusively, on the theoretical models of Norbert Elias and Jürgen Habermas, with assists from Max Weber, Thorsten Veblen, and others, Hubert Ch. Ehalt systematically analyzes a world in which the representational and ritualistic were politically and socially functional. Few of the work's formulations are new. Indeed, they are drawn almost entirely from monographic literature and printed contemporary tracts on the rationale of court practice. The author has, however, put them together with a sensitivity that comes only when one is fully at ease with the organization of the court, its ceremonies, its amusements, and its behavior codes.

It is true that Ehalt's enthusiastic conformity to structural patterns makes courtly society seem a good deal more bloodless than it actually was. His description of seventeenth- and eighteenth-century princely government as an extension of the patrimonial model with which the ruling house disciplined itself is accurate enough as far as it goes. It tells us nothing, however, of the endless personal tensions that the system provoked in individual ruling houses and that made courts far livelier places than Ehalt's overall presentation indicates. On the whole, however, his comments are sound

and sometimes insightful. Of such an order is his observation that wealth and power alone were not enough to confer status and prestige at the courts of the seventeenth century. Rather, these had to be displayed in a way that moved the sensibilities of others.

In meeting the objectives of the subtitle, the author is less successful. The actual treatment of the Vienna court is intermittent. The section on theater, opera, and amusements is very Habsburg-centered, but the tone of the work in its entirety is so sweeping that a reader frequently wonders if Ehalt is discussing courts in general or the Austrian one. He also seems constrained to justify his enterprise at the outset with claims that an understanding of court society will illuminate the social forms of today. As the book proceeds, however, this undertaking boils down to little more than noting the differences between court society and our own. These, again, are sometimes so sharply drawn that they distort the reality of his main concern, the court of baroque Vienna. For example, he seems to think that the decline of the representational in our day is due to the modern preoccupation with what men do rather than what they signify. This would seem to say that the seventeenth-century ruler and his noble attendants "did" very little, an implication totally unwarranted, if only by the high death rate in battle of the members of Europe's ruling houses during the era. Ehalt himself is probably aware of this, but such are the hazards of formulating historical problems in dualisms.

All of the above is serious criticism. It is intended, however, only to bring the true value of the study into sharper focus. Ehalt has certainly met his broad purpose very well, and any student of seventeenth-century government and of monarchy in particular should be acquainted with the book. Its bibliography is excellent.

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JOSEF EHMER. *Familienstruktur und Arbeitsorganisation im frühindustriellen Wien*. (Sozial- und Wirtschaftshistorische Studien, number 13.) Vienna: Verlag für Geschichte und Politik. 1980. Pp. 279. S 296.

During the nineteenth century, Vienna experienced the transformation of its economy from one dominated by the cottage industry and hand production to an advanced, industrial, factory system. The economic transformation had a profound impact upon the nature of Vienna's working-class families. Josef Ehmer has produced a work of high quality that describes the economic changes that occurred and shows the relationship between economic transformation and the nature of the worker family.

His book is based on the premise that in the development of society the family constitutes an element whose historical state of development and class nature determine society's function and form. An examination of the family, he maintains, provides a deep understanding of the nature and dynamics of society as well as a better insight into economics. Whereas earlier works have concentrated largely on describing the economy or society of Vienna, Ehmer contends that his effort demonstrates their interconnection. He proceeds by examining the preindustrial family in Vienna (prior to 1801), the era when artisans and cottage industry were dominant. Using the textile industry and selected areas of the city as examples, he describes the nature of work and the structure of the family in relation to economic productivity. The major focus is upon family structure, marriage and the formation of households, births and infant mortality, and household size.

The period of the Industrial Revolution in Vienna (1801–73) comprises the largest segment of the book. In it, Ehmer describes the major economic changes: the growth of new industries; the decline of the guilds, artisans, and cottage industry; and the impact of the changes upon the family, particularly the newly developing, working-class family. The period of industrialization, Ehmer contends, was a crisis for the working family and resulted in an altered life style, a decline in the age at marriage and the number of children, a changed parent-child relationship, increased opportunities for marriage, a reduction of the household size, and the employment diversity of family members. By the end of the Industrial Revolution, the traditional family in Vienna had disappeared and had been replaced by the working-class family in its modern form. During the period of high industrialization (post-1873), the rate of change for the family slowed greatly and did not alter its basic structure.

There is little in the book that is new in regard to the impact of industrial development upon the family, but the author builds a convincing case for the correlation of the rise of modern industry and the nature of the family in an industrial society. The greatest value of this work is the thorough description of the changes and its explanation of how the working-class family in Vienna was created and affected.

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ALFRED ERICH SENN. *Assassination in Switzerland: The Murder of Vatslav Vorovsky*. Madison: University of Wisconsin Press. 1981. Pp. xi, 219. \$21.50.

Late on the evening of May 10, 1923, Maurice Conradi, an anti-Bolshevik émigré, approached the

table of Vatslav Vorovsky in the Hotel Cecil dining room in Lausanne, Switzerland, and shot the unsuspecting Soviet diplomat. Vorovsky was killed instantly and his two companions were wounded, thus precipitating an international incident, the repercussions of which were felt from London to Moscow. The assassin made no attempt to escape, and his trial became a battleground between contending pro- and anti-Soviet forces. Alfred Erich Senn of the University of Wisconsin has painstakingly reconstructed this whole episode—from a highly dramatic recounting of the murder, through the varied public reactions to the crime, to the investigation conducted by Swiss authorities, and finally to the bizarre trial of Conradi and his co-conspirator Arkadius Polunin. Both the victim and the culprits quickly receded into the background of a broader ideological struggle. The Soviet government and the Comintern hoped to use the affair to discredit the anticommunist Russian émigré movement. Similarly, the attorneys for the defense attempted (with much more success than the plaintiffs) to transform the trial into an indictment of the Soviet regime by introducing lurid tales of the “red terror” and the supposed “nationalization” of women in the USSR. Attacking the Soviet system proved to be an effective defense, for despite their confessions both Conradi and Polunin were acquitted.

This volume carries on the theme of Swiss-Russian relations with which Senn has dealt in a number of articles and two previous books, *The Russian Revolution in Switzerland, 1914–1917* (1971) and *Diplomacy and Revolution: The Soviet Mission to Switzerland, 1918* (1974). *Assassination in Switzerland*, like these earlier works, is based on thorough research in archival and contemporary published sources. The author has used records of the Swiss political department, the federal police, the investigating magistrate, and an unofficial transcript of the trial as well as accounts from at least twenty Swiss, Western, Soviet, and émigré newspapers. Senn has seen fit not to annotate his book “in the interests of a more fluid style” (pp. x–xi), but he has provided an extensive chapter-by-chapter bibliographical essay.

In one sense, Vorovsky’s murder was only a minor incident in the tribulations of Soviet diplomacy during the 1920s, but Senn has been able to use the assassination in order to highlight a number of important themes. He explores the Soviet tendency, verging on paranoia, to see diabolically elaborate plots everywhere on the international scene. He gives us a picture of a curiously ineffective White émigré movement for which the “victory” in the Vaudois courtroom provided only a momentary emotional lift. “Certainly the assassination did not have the results that Conradi and Polunin had dreamed of. European politics were simply not at the flashpoint of war with Russia; the assassination failed to touch off an anti-Bolshevik crusade” (p.

192). Senn also has many interesting things to say about foreign perceptions of the Russian Revolution and about British policy toward the USSR at the Lausanne Conference. Beyond what can be learned from the book, it is quite simply a pleasure to read, since it has many of the qualities of a good detective yarn.

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EDWARD MUIR. *Civic Ritual in Renaissance Venice*. Princeton: Princeton University Press. 1981. Pp. xiv, 356. \$17.50.

In this important book Edward Muir is concerned not only, as his title suggests, with ritual but also with its frequent companion, myth, and not only with the Renaissance but with the whole of the millennium from the founding of Venice through the sixteenth century.

Muir deals with *civic* ritual and not, like Richard Trexler in his ambitious book on Renaissance Florence, with virtually all “formal behavior.” His subject matter includes Masses and processions, liturgy, Carnival, tableaux vivants, coronations of doges and their wives, funerals of doges, receptions of ambassadors, executions, and other public events. He believes that these rituals fostered harmony, patriotism, and the ideology of patrician republicanism both in the ruling class and in their potentially dangerous subjects.

The chapters on the medieval legends that were invented and then incorporated in the rituals are especially absorbing. There is room here for only one example of such mythmaking and its use. It is a fact that during the twelfth century Venice played a minor part in a war between Emperor Frederick Barbarossa and the Lombard League, and it is also a fact that, as a more or less neutral party, it provided the site for the eventual peace negotiations in which Pope Alexander III took part. But long after these events, Venetian chroniclers began to spin a fantastic tale. According to this legend the Venetians had played a major role in the war, at one point had saved Alexander’s life, and had then received from the grateful pope a number of symbolic gifts (a candle, sword, umbrella, lead seals, banners, and silver trumpets). Now ritual took over. Most of these objects sooner or later became fixtures in Venetian processions, their presence explained by the legend. As symbols they reminded the marching dignitaries and the onlookers alike (if they knew the legend) that Venice was loyal to the papacy, that the doge was the equal of popes and emperors, that St. Mark protected Venice in war, that Venice championed justice, and that a pope had formally recognized all of these pretended facts.

There are, of course, problems in ascertaining whether the rituals did indeed have the effect on the masses that their leaders more or less consciously intended. Did the gondoliers and sailors *understand* that the doge's umbrella symbolized his equality with other rulers? We shall never know, and of course we cannot ask Muir to prove the unprovable. In any case, there can be little doubt that the "people" absorbed the basic message of most rituals about the dignity and sacred character of their government.

During the Renaissance, Venetian public rituals increased in number, and Muir finds this surprising, since modernity is often believed to bring a decline in ritual and communal unity. He has particularly interesting things to say about the funerals of doges and the elaborate, ghastly punishments of traitors and murderers. Only Carnival, with its dangerous potential for violence, provides Muir with an awkward problem of interpretation. If the authorities were usually so concerned with order, why did they tolerate clownish mockery of the government on this occasion?

In studying his myths and rituals, Muir worked in seven archival collections and plowed through enough published primary and secondary sources to fill thirty-two pages of bibliography. More to the point, he has brought to his investigation highly developed research skills and a determination to see how everything fits together. He has made a major contribution to our understanding of Venetian stability and "serenity."

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ALESSANDRO PASTORE. *Marcantonio Flaminio: Fortune e sfortune di un chierico nell'Italia del cinquecento*. (Studi e Ricerche Storiche, number 10.) Milan: Franco Angeli Editore. 1981. Pp. 184. L. 9,000.

Marcantonio Zarrabini (1498–1550), whose father changed the family name to Flaminio, was a noted humanist and clerical member of the reforming party of the *spirituali*. Alessandro Pastore examines his life in the context of the intellectual and social history of the period, with special attention given to his hidden "heretical" ideas and to his dependence on prelates as patrons. Thus presented, Flaminio conforms to the paradigm delineated by Antonio Gramsci of the typical Italian intellectual of the early Reformation.

The industry of the author is evident. Pastore demonstrates a familiarity not only with the corpus of writings by and about Flaminio (especially his letters, which Pastore edited earlier) but also with a vast array of detailed studies on the culture of the times, notably the status of poetry, philosophy, and

science at the university centers of Bologna and Padua and the development of rabbinic and cabalistic scholarship in Italy. He thus provides a useful survey of certain aspects of High and Late Renaissance Italian culture. A number of relatively minor figures are also treated in significant detail.

Despite this erudition, the personality of Flaminio does not emerge from the text, and important questions are left unanswered. In his own eyes and those of his contemporaries, Flaminio was primarily a poet and stylist and a part-time philosopher, but Pastore never seriously analyzes these major dimensions of his work; the study by Carol Maddison is superior in these regards. Instead Pastore tends to give extensive information on the cultural context of Flaminio's writings without, however, showing how this relates to the works under consideration. The chapter on Hebrew scholarship is a prime example of this tendency. A more fruitful area of investigation would have been Flaminio's study of the Church Fathers. On the much-debated question of Flaminio's role in the composition of the *Beneficio di Cristo*, Pastore declares current scholarship inadequate on basic issues but then moves on without making his own contribution.

More problematic still is Pastore's attempt to explain Flaminio's conduct in terms of the Gramsci model. Although this humanist did concern himself with the wider theological questions of his day to the neglect of issues focused on Italian civic life, it is not at all clear that he allowed himself to be "suffocated and domesticated by the Church" instead of taking a decisive and open stand in Italy on the side of the Protestant Reformation. Pastore defines Flaminio as a crypto-heretic on the sole issue of justification, yet he never examines the spectrum of tolerated Catholic opinion or recognizes that many Catholics in the 1540s—notably northern rulers—did not regard the recent Tridentine decrees as definitive. Instead of being viewed as a cowardly cleric who hid his heresy for reasons of convenience and material gain, Flaminio, who had applied for membership in the Theatines, denounced novelties in theology, and read Protestant authors with discernment (*tamquam aurum ex stercore colligentes* [p. 122]), can perhaps better be seen as a sincere Catholic who had a deep personal trust in Christ and cautiously sought reconciliation with Protestants on the key question of justification by faith. His degree of dependence on clerical patrons is never clearly established. Given his multiple benefices, it is doubtful that his basic needs went unmet. But this son of a schoolteacher wanted a life of leisure and comfort and the companionship of the most cultured people of his day. These he found by joining the households of such important prelates as Ghiberti, Pole, and Farnese. Pastore never seriously examines the responsibilities or revenues (if any) that fell to a familiar, the

frequency and financing of Flaminio's peregrinations (how sickly was he apart from periodic bouts with malaria?), and the constraints that a patron could place on a noted humanist who brought learning and prestige to his household. That Flaminio was suffocated and domesticated by the church is yet to be proven.

NELSON H. MINNICH
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MARINA ROGGERO. *Scuola e riforme nello stato sabaudo: L'istruzione secondaria dalla Ratio Studiorum alle Costituzioni del 1772*. (Biblioteca di Storia Italiana Recente, new series, number 18.) Turin: Deputazione Subalpina di Storia Patria. 1981. Pp. 312. L. 15,000.

In 1729, Victor Amadeus II of Piedmont decided to remove secondary schools from the control of the Jesuits and to establish a network of state colleges. The new schools were not reserved to the nobility but rather were intended for the education of a rising bourgeoisie that the king was seeking to attract to administrative positions in the Piedmontese state. Some fifty years later, however, the intentions underlying the reform of 1729 had been all but forgotten. Why?

In *Scuola e riforma nello stato sabaudo*, her latest book on Italian education, Marina Roggero attempts to answer the question by looking at preceding educational conditions as well as at the intentions, consequences, and limits of the 1729 reform. Roggero makes the convincing argument that the establishment of the royal schools after 1729 reflected a concern on the part of the nascent bureaucratic state to organize—under its own control and on the basis of a new uniformity—a school system for the middle class. In essence it reflected a new alliance between the absolute monarch and the middle class.

But the success of the Piedmontese reform was tempered by two factors: the availability of qualified teachers and funds. Also, the reform of 1729 strained the religious and cultural ties that had linked the old schools to local communities. The shock was too severe for local particularism to bear. Conflict was heightened at all levels, and the state simply did not have the means to enforce its reformist program. Moreover, the state never came out openly against the role of religious orders in the moral education of youth. School administrators were forced to perform a balancing act: respect church authorities and defend the autonomy of the royal schools. It was only a matter of time before the status of royal schools became more and more ambiguous. Indeed, by 1772 all private—including religious—schools were recognized as long as they met certain basic criteria.

Paradoxically enough, the reform of 1729 was being eroded from within precisely at a time when the Jesuits were being expelled from other states. This, argues Roggero, implies not only that the reform of 1729 had given rise to a whole series of provincial and reactionary forces that did much to thwart the original intentions of the reform but also that Piedmont was still on the fringes of Enlightenment thought and action.

Needless to say, the book is a welcome contribution, reflecting mastery of the sources on eighteenth-century Piedmontese education and skill in the analysis of often difficult-to-obtain data. What we have here is the social history of Piedmontese secondary education and culture, as well as a political history of church-state relations with respect to education. Roggero's analysis of Piedmontese secondary schools is a useful prism through which to view the evolution of Piedmontese absolutism from the promising reformist era of the 1730s to the regression at the end of the century. This book should prove valuable to anyone interested in eighteenth-century education, absolutism, church-state relations, and culture.

ELMIRO ARGENTO
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VICTORIA DE GRAZIA. *The Culture of Consent: Mass Organization of Leisure in Fascist Italy*. New York: Cambridge University Press. 1981. Pp. x, 310. \$34.50.

This study of the Italian Fascist leisure-time *dopolavoro* organization is a highly competent, if somewhat tendentious, monograph. It explains how Mussolini tried to create a popular consensus for his regime by developing "depoliticized" recreational programs for industrial workers, peasants, and salaried employees that appealed to—and at times molded—nonclass interests. The result, Victoria de Grazia maintains, was an unprecedented state intervention in mass cultural life and the emergence of a new "politics of culture." The research is admirable, for it is based on archival collections, periodicals and tracts of the period, and recent scholarly contributions.

Some readers may be swayed from the merits of this work only by the enthusiasm with which de Grazia embraces a particular interpretive approach. She attempts to conceptualize the role and function of the *dopolavoro* within an analytical framework derived from the concepts of Antonio Gramsci, to whom she carefully documents her intellectual indebtedness. Hence, she argues that "the organization of consent may be identified as an essential aspect of the achieving of class hegemony" (p. 21) and advises her readers that "the organization of a

culture of consent, as it is analyzed in this book, *must* be understood first of all as involving the self-organization of Italy's capitalist classes" (pp. 21–22, italics added). Her preface places Fascism's leisure-time activities in the context of "the institutional and cultural transformations that occurred throughout Europe and America as the immediate corollary of the reconstruction of bourgeois states under monopoly capital" (p. viii). Fortunately, it is possible to separate de Grazia's excellent research from the constraints of this self-acknowledged limitation.

Arguments concerning the nature of the consensus achieved through the *dopolavoro* are not so easily resolved. The problem of "consensus" has in fact been central to the historiographical debate on Fascism ever since the publication of the later volumes of Renzo De Felice's biography of Mussolini. The author appears to agree with De Felice that the regime achieved a considerable degree of consensus, that it reached its apex at the end of the Italo-Ethiopian war, and that it declined rapidly over the next few years. Naturally, de Grazia contends that the *dopolavoro* contributed significantly to that early success, largely because it reinforced values already held by segments of the population that the regime had "officially sanctioned" and "appropriated" (p. 220). The difficulty, of course, is that in a dictatorship reliable indices of popular attitudes do not exist, so that historians must make deductions and assumptions based on circumstantial, indirect evidence (including cultural content analysis, institutional membership patterns, contemporary commentary, police reports, and official policy statements). De Grazia does just that, and her conclusions must therefore be regarded, like those of other scholars who have tackled this thorny problem, as tentative.

PHILIP V. CANNISTRARO
Drexel University

PETER C. KENT. *The Pope and the Duce: The International Impact of the Lateran Agreements*. New York: St. Martin's Press. 1981. Pp. ix, 248. \$22.50.

Both the strengths and weaknesses of this study by Peter C. Kent lie in the close attention given to primary sources, especially the Italian diplomatic documents and the papers of the British Foreign Office as well as a collection of Italian documents at St. Antony's College, Oxford, and other papers and interviews. Missing from this array, through no fault of the author, are Vatican documents. That gap will presumably remain for some decades, so it is useful to have this piece of diplomatic history done even without them.

On Mussolini's side of the story, Kent succeeds in showing that the rest of Italian foreign policy was

not constrained or particularly influenced by the entente with the Vatican sealed in the Lateran Agreements of 1929. Nor did the Duce really attempt to exercise massive influence on the Vatican's own diplomatic endeavors. There seems to have been a practical acknowledgment that the papal concerns were largely of a different order ("pastoral"), so that the clash between Mussolini and Pius XI, when it came, was basically a domestic one that just spilled over as far as Yugoslavia. A summation on page 120 emphasizing the pope's capitulation in 1932 is perhaps not quite sustained by the facts presented.

The weakness of Kent's approach is his overreliance on Foreign Office reports for Vatican attitudes and measures. For background he resorts to Carlo Falconi. He could have found still more pertinent sources and contextualization in Hansjakob Stehle's *Eastern Politics of the Vatican* (published in German in 1975), especially with regard to Vatican contacts with Moscow and Belgrade (Kent, pp. 25, 53, 112–14, 129). Stehle also helps one see what there is to Kent's contention that Pius XI's turn to Fascism as a defense against the Bolshevik threat dated not so much from 1920 in Warsaw as from the depression situation. Kent, however, provides an important bit of evidence by translating Mussolini's *Minute* of his meeting with Pius XI on February 11, 1932.

The book ends on the note of satisfaction in the Vatican at the Franco-Italian rapprochement (with Laval) of January 1935; failure to examine the repercussions of the Ethiopian campaign and the Spanish Civil War, both of which occurred while Pius XI was still alive, raise questions about that choice of a stopping place.

PAUL MISNER
Marquette University

ARNOLD TOYNBEE. *The Greeks and Their Heritages*. New York: Oxford University Press. 1981. Pp. x, 334. \$19.95.

Arnold Toynbee's monumental ten-volume publication, *A Study of History*, made him a world figure and one of the most controversial writers of his generation. Toynbee's vast learning enabled him to examine the history of the human race by concentrating on the rise and fall of some twenty-one civilizations. One wonders what captivated Toynbee's mind when, on the summit of Mithra's citadel, he pondered over "the cruel riddle of Mankind's crimes and follies." He was overwhelmed by "a horrifying sense of the sin manifest in the conduct of human affairs." Each civilization, he asserted, rose in response to an external challenge, and he added that our Western civilization's only hope lay in the cre-

ation of a world state and the concomitant establishment of a new world religion blending Christianity, Islam, Hinduism, and Mahayana Buddhism. Confronted by bitterly hostile critics, Toynbee maintained a dispassionate temperament. In a regimented age, he believed, religion may be man's great opportunity for freedom. He saw history, in the Jewish-Christian-Muslim Weltanschauung, as being placed in a framework of theology, a viewpoint rejected by Western historians. To Toynbee history leads toward the *visio beatifica*.

The present volume, *The Greeks and Their Heritages*, may not match Toynbee's magnum opus, *A Study of History*; it is, nevertheless, a lofty achievement and a personal testament. Undoubtedly, the work's challenging theses and canonical viewpoints, bordering on tautologisms, will again stir controversy. His "Byzantine-like" education, combined with wanderings throughout the length and breadth of the Greek world, gave him a keen understanding of Greece and its people. This, in turn, produced an intense desire to illuminate both the faults, as well as the virtues, of the Greeks in their various stages of historical development. His principal concern was to discover why the Greek people's "reactions to their heritages differed so greatly at different stages of their history."

After commenting briefly on the influence of heritages from the past and appraising the Mycenaean Greeks' successes and failures, Toynbee concluded that the classical Hellenes managed to escape from being dominated by the legacy of their predecessors. Unencumbered by tradition, the Hellenic Greeks reshaped and modified the Mycenaean image in order to adapt it to their "constantly changing fashions of Hellenic life," which accounted for their vitality and relative success. In fact, the classical Hellenes "dominated the Mycenaeans retrospectively."

In contrast to the classical Hellenes, both the Byzantine Greeks and the modern Greeks were overshadowed by the crushing heritages of their progenitors, and this burden led to their premature collapse. The Byzantine Greeks were unable to escape from such legacies as the "incubus" of the Eastern Roman Empire and the Hellenic *paideia*, which acted as strangleholds on the Byzantines "and they paid dearly for their servitude to the past." While the imperial government was overstraining the economy, the Hellenic *paideia* was casting a hypnotic spell over Greek men of letters. The Attic *koiné* was preferred to the contemporary form of living Greek as the language of both hymnography and hagiography. Although some vernacular was produced in an accentual meter, Greek literature was "inhibited by the dominance of a literary heritage in a dead form of the language."

From their Byzantine forebears, the modern

Greeks inherited the twin anathemas, the language question, and the Great Idea. These, combined with the anti-Western, xenophobic attitude of the Byzantine church, thwarted the modern development of the Greek nation. Only after many encounters was the ecclesiastical antipathy to the West resolved by the Greek people, whose determination to reject the church's misgivings and to pursue an identity in the Western tradition enabled them to break free from their Byzantine past.

Toynbee assessed both successes and failures of the modern Greeks. Although their struggle for independence from the Ottomans did, at last, produce the modern Greek state, the modern Hellenes, like their revolutionary forebears, have been less than successful in directing their domestic affairs. Toynbee makes the unique assertion that the Greeks' noteworthy engagement in the commercial life of the modern world has been unprecedented, and he compares it with the Hellenic Greeks' intellectual and artistic achievements. Responding to the Greek emotional reaction over the unacceptable loss of the imperial city, Constantinople, Toynbee remarked that the loss relieved them of the Byzantine nightmare. The language question still plagues Greece and constitutes "the supreme example of the bewildering and inhibiting effect of the Greeks' heritages from the past on Greek life."

The study terminates with a three-page conclusion, four appendixes, a selective bibliography, and an ample index. In the appendix on the post-Mycenaean dark age, Toynbee showed how the Hellenic Greek tradition of appending the Mycenaean Age to their own is irreconcilable with recent archaeological discoveries. The post-Mycenaean *Völkerwanderung*, he declared, contrasts with its post-Hellenic counterparts. In another appendix, he labeled George Yemistos Plethon's totalitarian Hellenism a *reductio ad absurdum* but, simultaneously, a "posthumous vindication of the Hellenists' ideology."

At times Toynbee's interpolations and bold interpretations are characterized by vitality and freshness, but on occasions they appear tediously repetitive. Toynbee, nevertheless, impresses one with his brilliant and debatable judgments, and his breadth of vision is discernible in his penetrating assessment of Greek history. One can merely give a perfunctory indication of this study's importance in a restricted review. Greek scholars may quarrel with its synthesis, but they cannot deny the study's significance. It should become the subject of a scholarly conference.

WILLIAM PETER KALDIS
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MORITZ CSÁKY. *Von der Aufklärung zum Liberalismus: Studien zum Frühliberalismus in Ungarn.*

(Veröffentlichungen der Kommission für die Geschichte Österreichs, number 10.) Vienna: Verlag der Österreichischen Akademie der Wissenschaften. 1981. Pp. 272.

The scholarly literature on Hungary's age of reform is voluminous. Moritz Csáky claims, however, that this era's background has been neglected. It, too, merits investigation, both for its intrinsic importance and because it typified the evolution of liberalism elsewhere in Europe. Utilizing archival repositories in Budapest and Vienna, various printed documents, and predominantly German and Hungarian secondary sources, Csáky examines late eighteenth-century Hungarian society and traces its proto-liberal transformation, beginning with the Reform Diet of 1790–91 until just before Count István Széchenyi's arrival on Hungary's political scene in 1825.

Four sections depict various aspects of what the author terms late Enlightenment or early liberalism. Section 1 considers Hungarian government and society; section 2 traces the growth of Hungarian political consciousness and raises constitutional questions; section 3 addresses educational reform; and section 4 weighs the repercussions of the Enlightenment on Hungarian development.

Csáky illuminates Hungary's administrative, demographic, and social structure before examining attempts by the Hungarian diets, the Hungarian Jacobins, and various reform-minded individuals to liberalize the country's anachronistic institutions. He also depicts the power struggle that pitted Hungary's autonomist nobility against Habsburg centralism and explains how Napoleon Bonaparte's involvement in Hungarian affairs further exacerbated Austro-Hungarian relations.

Csáky highlights Austrian attempts to rationalize and standardize the monarchy's school system. Maria Theresa's *Ratio educationis* of 1777, Joseph II's modified formula of 1782, and Francis I's revised *Ratio* of 1806 failed, however, either to democratize or to systematize Hungarian education. But these Habsburg initiatives did portend the government's determination to monopolize the monarchy's instructional system. Csáky shows how the Hungarian diets and assorted Hungarian pedagogues thwarted Austria by championing a system they termed *educatio nationalis*. Under it, all social classes would enjoy equal educational opportunities. These and other controversies simmered beneath the surface throughout this period.

Csáky maintains that in Hungary the lines separating the Enlightenment and liberalism were blurred. Early liberals were not rigid but drew selectively from various strands of Enlightenment thought. Later, many of these adaptations coalesced into Hungary's liberal mystique.

This study only partially fills the topical void the author claims to have detected. As an effort to trace Enlightenment influence in late eighteenth-century Hungary, the book scores. It falters, however, when it tries to explain how the feudal Hungarian establishment metamorphosed into a Magyar-dominated, liberal polity. Csáky should have investigated the role of Magyar nationalism (which was stimulated by the Magyar linguistic revival), demands for Magyar education, the burgeoning of Magyar patriotic themes, and the galvanizing effects German romanticism had on Magyar nationalistic literature. Moreover, had Csáky perused the English-language monographic literature more thoroughly, he would have discovered that Hungary's pre-reform era was not as badly neglected as he feared.

These cautionary comments aside, specialists in Austro-Hungarian and Enlightenment history will profit from this intelligently argued, well-written, and factually accurate scholarly work. But an authoritative monograph encompassing all the essential elements of the early stages in Hungary's age of reform has yet to be written.

THOMAS SPIRA

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ZDENEK KRYSTUFEK. *The Soviet Regime in Czechoslovakia*. (East European Monographs, number 81.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. vii, 340. \$21.00.

As the author put it, he wrote the book because "he considered it his duty to place at the disposal of experts in sovietology the results of a direct observation of the soviet regime in Czechoslovakia from outside the communist party . . ." (pp. vi–vii). It seems, however, that on occasions he himself has been a victim of the Communist propaganda. He writes, for example, that the inhabitants of Prague, who were fighting the Nazi occupants in May 1945, were forced "to wait for Soviet help" (p. 10). The truth of the matter is that the Soviet army arrived *after* the city of Prague had been liberated by the Vlasov army. Another example is his using the phrase "the capitalist dictatorship in the extreme form of Nazism" (p. 192). In fact, the NSDAP (National Socialist German Worker's party) rejected capitalism, denounced "plutocracy," and created a socialistic economy.

In Zdenek Krystufek's discussion of the Czechoslovak economy, his comments on legal education, the legal system, the power elite, and the role of fear are the most informative parts of the book, which consists of 197 pages of text and 80 pages of notes. In addition, there are 61 pages of bibliography, appendix, and index. The work is based mostly on

books and articles. Among the useful quotes are those related to the events in the 1950s and 1960s.

Although Krystufek says that his field of specialization is "the history of legal and political philosophy" (p. vi), his treatment of theoretical issues lacks clarity and is confusing. He does not define his terms. Thus, in the chapters on "Socialism and Democracy" and "The National Front" one searches in vain for the relevant definitions. He does not seem to realize that there are many different kinds of socialism and democracy. For example, socialism may be Marxian, non-Marxian, utopian, or democratic; democracy may be direct, representative, participatory, liberal, pluralist, or Marxist-Leninist. He does not explain the theory and practice of the National Front. His starting point seems to be the year 1948, and by that time the National Front had a long history.

There is no mention in the book of the techniques used during the process of the Communist seizure of power, as, for example, the launching of the process of self-liquidation within the non-Communist political groups, a technique that the Polish Communists are trying to apply against Solidarity now. There are no references to the role of President Eduard Beneš during and after the war, to the elimination of political parties representing the majority of the prewar electorate by the Košice program of April 1945, to the expulsion of three million Sudeten Germans, or to the economic and political consequences of those events.

Krystufek does not notice the continuity of the process that began during World War II and its culmination in the February 1948 events. He does not realize the significance that signing the treaty with the Soviet Union by Beneš in December 1943 had for the pro-Soviet orientation of the country after the war. He is unaware of the commitments made by Beneš to the Czechoslovak Communists and Stalin in December 1943 and later. At one place he does refer to a source containing some of the relevant documents, *The Way to May . . . 1945*, but he clearly did not read it (p. 17), for he gives V. Král as its editor (pp. 211, 280). In fact, the editors of the documentary collection are Miloš Klineš, Petr Lesjuk, Irena Mala, and Vilém Prečan. (Incidentally, Krystufek does not give the first names of any editors of books listed in the notes and bibliography.) The name of "the Beneš Socialist" party (p. 215) was National Socialist, and the name of the "Slovak Catholic" party (p. 194) was the Slovak People's party. The many awkward and unidiomatic expressions do not enhance the value of the book, either.

Without offering any proof, Krystufek writes that "Stalin advocated the concept of a world divided into two spheres of interest" (p. 8) and that Franklin D. Roosevelt accepted "the concept of divided

spheres of interest" (p. 9). He fails to notice that Czechoslovakia was not an issue at the Yalta conference or in the "percentage deal" of Winston Churchill with Stalin, though he quotes that British statesman extensively (pp. 200–01). Neither does he notice that the Communist party of Czechoslovakia had been tied by an umbilical cord to the Soviet Communist party since 1929, an observation that is essential to understanding that party's behavior in 1948 and 1968.

JOSEF KALVODA
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ANATOL LESZCZYŃSKI. *Żydzi ziemi Bielskiej od połowy XVII w. do 1795 r.: Studium osadnicze, prawne i ekonomiczne* [The Jews of Bielsk Province from the Middle of the Seventeenth Century to 1795: A Study of Settlement, Legal, and Economic Problems]. Summary in English. (Żydowski Instytut Historyczny.) Wrocław: Ossolineum. 1980. Pp. 276. 45 Zł.

Given the number of times since 1945 that large numbers of Jews have made an exodus from Poland, one hardly expects to find published in the Poland of 1980 a monograph on Polish-Jewish history that draws on sources in Yiddish and Hebrew as well as in Polish and Russian. Yet it is precisely this kind of linguistic and historical expertise, which was much more common in the 1960s, that marks Anatol Leszczyński's study of rural Jews in the Białystok area of northwestern Poland in the seventeenth and eighteenth centuries.

The study is all the more welcome in that it describes small-town life in an age about which we know little. Historians of Poland have been concerned primarily with the formation of the Polish nation when dealing with this time period. Where they have turned to social stratification, they have emphasized the upper classes or the peasantry, but they have in the main neglected the extensive national and ethnic minorities within the Polish Commonwealth.

Leszczyński limits his monograph to the small towns of the Białystok area, drawing on local archives for lists of residents, for their occupations, and for other local data, mainly pertaining to their legal or economic status. He also refers extensively to Yiddish memorial volumes devoted to individual communities, based on now unavailable sources. He starts with the legal status of the Jews in these towns and villages, a subject necessary to define the subject. He then devotes one chapter to artisanry, innkeeping, and brewing; one to trade; and one to credit operations and leasing of estates from Polish gentry and nobles. These constitute a standard list of topics for the economic history of Jews in Eastern

Europe, but the author can present much more detail than ordinarily provided because he has chosen a small but typical area of Jewish settlement in Poland.

On the other hand, this standard item-by-item approach robs the text of the dynamism that might be found in an account of social conflict. The picture painted here is relatively flat, without the nuances that certainly could be drawn from this historical situation. Still, this monograph is a short one, intended to present data culled from practically inaccessible archives and available in a variety of languages not widely known in Western Europe and the United States. The topic is also one in which there is very little scholarly interest in Poland today. The study would serve as a useful building block for the history of Jews in Poland and the economic history of Poland itself, if only we could be sure that the kiln for these blocks would continue to exist and that more masons could be trained.

LAWRENCE SCHOFER
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ROBERT C. RIDENOUR. *Nationalism, Modernism, and Personal Rivalry in Nineteenth-Century Russian Music*. (Russian Music Studies.) Ann Arbor: UMI Research Press. 1981. Pp. 258. \$34.95.

Belief in an integral relationship between the arts and society is a fundamental part of much Russian culture. True, the father of Russian music, Mikhail Glinka, seems to have been little troubled by it, relying simply on a profound Russianness fostered by biographical accident and an uncommonly brilliant imagination as the sources and shapers of his creations. But the generation following Glinka, the dazzling collection of musical talents that sprang into activity in the 1860s, was often profoundly concerned with this matter. *Boris Godunov* would never have turned out as it did, had not Mussorgsky held passionate views on the condition of Russian society, and Balakirev was constantly exercised by the question of how Russian music should evolve and actively promoted a "progressive" nationalist art in opposition to the traditionalist Westernizing party. Without an awareness of such deep divisions of belief, the music of the 1860s and 1870s cannot be fully appreciated.

Robert C. Ridenour's book is a study of the fifteen or so years of musical life in St. Petersburg following the foundation of the Russian Musical Society (RMS) in 1859. All music historians of the period know something of what ensued—of how, besides giving concerts, the RMS swiftly expanded its teaching activities to create the St. Petersburg Conservatoire in 1862. They also know something of the activities of Balakirev and his group who, through

the Free Music School, hoped to counter what he saw as the pernicious influence of the ideals and compositional practices peddled by Anton Rubinstein at the Conservatoire. But there was far more to the situation than the simple Rubinstein-Balakirev opposition. Indeed, as Ridenour reminds us, the Free Music School was founded not by Balakirev but by Gavriil Lomakin as an institution intended primarily to teach singing. Balakirev's motives were not completely altruistic, as we often tend to think; nor, for that matter, was Rubinstein simply the reactionary agent of the establishment. And what of the role of Alexander Serov, to many Russians of the 1860s the greatest living native composer as well as a critic with a potent and influential pen? What of the roles of Vladimir Stasov, Cesar Cui, the Grand Duchess Elena Pavlovna, and of lesser figures like Famintsin, Feofil Tolstoy, and Laroche? (For in the end, despite all the varied beliefs and ideals that exercised the participants, it was their personalities and ambitions that often really dictated the course of events.) Such matters are the very substance of Ridenour's book, and his narrative makes clear the powerful role of aggressive partisanship and plain self-interest in these turbulent years. His exposition is clear and readable (though he has a tendency to repeat himself), and his account of the complex web of historical events generated by these personalities is comprehensive. A shadowy but crucial social background to a vital period of Russian musical creativity is now brought more clearly into focus.

DAVID BROWN
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NEIL B. WEISSMAN. *Reform in Tsarist Russia: The State Bureaucracy and Local Government, 1900–1914*. New Brunswick, N.J.: Rutgers University Press. 1981. Pp. x, 292. \$19.50.

Neil B. Weissman considers local government to have been not only an enduring problem in Russia, in particular a major one for the last tsarist government, but also an interface between government and society, one of the arenas of the "we-they" controversy. After reviewing the inadequacies of both the local administration and local self-government in the late nineteenth and early twentieth centuries, Weissman examines the proposals to improve local government launched by Pleve, Witte, and Stolypin. All variations on a theme, in their final form they included measures to strengthen the governor, making him the true manager of local government; creation of a vice governor and council on the district level to supplant the *uezd* marshal of nobility; abolition of the poorly filled office of *zemskii nachal'nik*; institution of *volost'* and settlement *zemstva* that would include all local estates but allow

tutelage on the part of "cultured" and politically reliable nobles; and increased official accountability. Weissman constantly emphasizes that Pleve and Stolypin adhered to the "state" theory of government—allowing a modicum of popular participation in local affairs but requiring supervision and control by the local and ultimately the central administration.

Describing local government as a triad consisting of the local administration, local self-government, and the noble estate, Weissman balances his study of governmental projects with examination of responses to them on the part of various ministers and local interest groups—*zemstvo* figures and gentry—both at the grass-roots level and in the Duma. He particularly spotlights Stolypin's relationship with local groups in the *Sovet po delam mestnago khoziaistva*. Finally, Weissman portrays a decline of governmental interest in local government reform between 1911 and 1914, save for half-hearted attention to overhauling the overburdened, underpaid police.

I was chagrined to note that my book, *Peter Arkad'evich Stolypin: Practical Politics in Late Tsarist Russia*, was accorded only one footnote, and that on a trivial subject. Yet Weissman's discussion of Stolypin's proposals for local government, opposition to them in the Council of Ministers, and the western *zemstvo* crisis (pp. 127–44, 154–58, 198–201) is nearly identical to my coverage of these topics (pp. 51–69, 174–79). I also presented a condensed analysis of the *Sovet po delam mestnago khoziaistva* (pp. 69–73) and information on the composition of the provincial and district councils that Weissman omits. Weissman's discussion of the position of the Poles in the western provinces is superficial and trite. Furthermore, because he is focusing on local government reform, Weissman makes it appear that Stolypin was almost totally occupied with this issue, whereas in reality Finland, the naval general staff crisis, budget conflicts with the Duma, and Stolypin's relationship with the tsar were equally important. Weissman's avoidance of Russian terms is as annoying as his "liberal" bias.

On the positive side, Weissman confirms that some bureaucrats in Nicholas's government were reform-minded, albeit not committed to fully sharing power with extragovernmental groups. He underlines the fact that Stolypin had more on his mind than the hackneyed agrarian reforms. Although somewhat impressionistic and repetitive, Weissman's study of interaction between the central government and local groups adds to our knowledge of the dynamics of Russian government in the late prerevolutionary era. Some interesting specific contentions are (1) that nobles could not be ancillary to local officials because nobles had been hurt by the late nineteenth-century agrarian crisis and were moving to cities and (2) that Stolypin modeled his

projects on French local government. Finally, Weissman has explored the nearly untouched subject of police reform. The book is well written, carefully researched, and a valuable reference tool.

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A. D. MALIAVSKII. *Krest'ianskoe dvizhenie v Rossii v 1917 g.: Mart-oktiabr'* [The Peasant Movement in Russia in 1917: March through October]. Moscow: Nauka. 1981. Pp. 399. 2 r. 50 k.

E. D. POPOVA. *Leninskaia agrarnaia programma i bor'ba bol'shevikov za krest'ianstvo v 1917 godu* [The Leninist Agrarian Program and the Struggle of the Bolsheviks for the Peasantry in 1917]. Leningrad: Leningrad University Press. 1980. Pp. 129. 65 k.

At national conferences held in Moscow in 1967 and in Sverdlovsk in 1969, Soviet historians sharply rebuked a minority of their colleagues for "uncritically" arguing—and in contradiction with Lenin's pronouncements—that in 1917 the Russian proletariat allied with the *entire* peasantry rather than with its poorest stratum to smash the old order and fashion socialism. This heated, if at times unexciting, debate reminded Western historians that historiographical issues do flare up in Soviet historical literature and that the role of the peasantry in 1917 remains one of the least-explored aspects of the revolution. The two books under review stand as testimony to the nature of the above-mentioned controversy as Soviet scholarship enters the 1980s.

E. D. Popova's slim, narrowly focused volume traces the evolution of the Bolshevik agrarian program from the formation of the party through the October Revolution. Much more importantly, however, the book serves to reinforce the now orthodox interpretation that the poorest peasantry marched in step with the working class in 1917. Although Lenin did view the peasant poor (*bedniaki*) as a potentially revolutionary force, this theme did not prevail in Soviet publications of the 1920s and seemingly did not become an issue until the publication of P. N. Sobolev's *Bedneishee krest'ianstvo—soiuznik proletariata v Oktiabr'skoi revoliutsii* (1958). Stressing the scientific character (*nauchnost'*) of the Bolshevik agrarian program and the organized nature of the peasant movement, Popova leaves no room for opportunism or political expediency. All in all, her monograph has no surprises and clearly serves a polemical purpose.

Putting Popova's work in sharper focus, A. D. Maliavskii's ambitious investigation merits our attention: it is the first attempt since S. M. Dubrovskii's 1927 publication to chronicle all aspects of the

peasant movement for the whole of European Russia, and to a lesser extent for Siberia and the Far East. Carefully plumbing central archives and effectively drawing on the large number of regional studies, document collections, and monographs published in the past two decades, Maliavskii has produced an important work of synthesis whose aim is to characterize the dynamics, peculiarities, and forms of the peasant revolt in 1917.

Arguing that the peasant movement was more massive and more organized than most Soviet writers conclude, Maliavskii tackles the thorny problem of offering a methodology for counting peasant disturbances (*vystupleniia*). He identifies 44 types of disturbances, establishing criteria for distinguishing them. Maliavskii draws an insightful parallel between workers' control at industrial enterprises and the peasant committees' attempts to regulate the land, equipment, livestock, and production of the large estate owners. He also insists that the "second class war" in the countryside greatly surpassed the estimates of most Soviet writers. In line with the new consensus on the union of the proletariat with the poor peasants (who, according to the author, made up 65 percent of the entire peasantry), Maliavskii holds not only that the peasantry as a class was pitted against the landlord but also that within the peasantry the poor elements clashed with the well-off kulaks. Like V. I. Kostrikin and G. A. Gerasimenko, Maliavskii sees the *volost'* executive committees as the driving force in the countryside. Throughout the narrative he points out gaps and historiographical issues in contemporary Soviet writing. He also provides an interesting discussion of the activities of farm day laborers (*batraki*) and criticizes historians who characterize the peasant uprising in the fall of 1917 as anarchistic, marked by the indiscriminate seizure of estates. Nine detailed tables on various aspects of the peasant movement further enhance this work.

Despite its merits, the study has most of the weaknesses we have come to associate with Soviet scholarship. Maliavskii distorts the intent of several Provisional Government circulars. He shows no understanding of Western scholarship (his mandatory slap at "bourgeois falsifiers" strikes the "contemporary Sovietologist" [!] John Maynard). The book deals only tangentially with the question of political power and the various crises of the successive coalition governments. Although he places great emphasis on peasant organizations, Maliavskii's narrative devotes disappointingly little attention to their fate in the fall of 1917. The major flaw—common to all Soviet works that crudely classify peasants into poor, middle, and rich categories—surrounds the middle peasants, who remain murky figures and for the most part indistinguishable from the poorer villagers. Finally, the author gives but passing attention to the link between the

army and the peasant movement or to the changes in the commune noted by some writers. He does not analyze spontaneous forces described in available documents and only superficially touches upon landowners' organizations and the economic repercussions of the peasant movement. He ignores the Orthodox church altogether. Nevertheless, Maliavskii's book is one of the best Soviet works on the peasant movement to appear in the past decade.

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GEORGE LEGGETT. *The Cheka: Lenin's Political Police; The All-Russian Extraordinary Commission for Combating Counter-Revolution and Sabotage (December 1917 to February 1922)*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xxxv, 514. \$29.95.

This is the most thorough and careful study yet produced of the Extraordinary Commission to Combat Counterrevolution and Sabotage, the forerunner of the GPU, NKVD, and KGB. Created a few weeks after the October Revolution of 1917, the Cheka led a fitful existence as the "shield of the revolution" and the instrument of Soviet terror. Improvised as an institution with ill-defined powers, it emerged as an inchoate but potent empire within a disorganized state, engaged, until its reorganization in 1922, in a vast array of activities from labor conscription to strike-breaking, from crushing peasant revolts to ferreting out imaginary enemies. This detailed volume, based on painstaking research, is the work of an informed and intelligent student. It covers a good deal more than just the Cheka: this is a political history of the Lenin years, with fair attention to the major (and some minor) actors on the Soviet scene.

Why did scholars avoid this topic for so long? Perhaps because much that was published was unreliable or sensationalized; perhaps because, broadly speaking, primary sources were lacking. In the past generation Soviet publication (however selective) of documents, monographs, and memoirs has filled some gaps—largely to glorify the pre-Stalinist record of the early Red Terror. George Leggett has made fine and generally sensible use of these materials as well as of the scattered sources abroad. He argues correctly that terror was not a logical product of Marxist ideology but a natural departure from doctrine for the sake of consolidating power, a consequence of the forcible takeover by a determined minority. Did the bloody experience of the Cheka under Lenin, in turn, make Stalinist terror and abuse inevitable? Leggett suggests they did not have to follow, but the Cheka laid the foundations for them.

The major strength of the book lies not in its original interpretation but in the rich detail and realistic picture it conveys. The author documents the variations in the attitudes of leading Bolsheviks toward terror (with Kamenev and Bukharin often on the side of the skeptics, and Trotsky and Stalin among its backers, but with interesting variations and vacillations on the part of most, including Lenin). He is excellent in tracing the institutional infighting between the Cheka and various rival agencies—such as the Interior and Justice ministries, the Red Army, and the Party—as they vied for power and authority. Felix Dzerzhinsky, the Cheka's head, emerges as a human being far more complex than merely a fanatical executioner.

If there is occasional verbal hyperbole, the topic invites it. The errors seem minor (for example, the Orel and Orlov Cheka are the same, and it is not clear on what basis Genrikh Yagoda or Stanislaw Messing are found to be “almost certainly” Jewish). The detailed biographical sketches are valuable. All in all, the work will be important for future students—Soviet scholars included.

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NEAR EAST

ROGER OWEN. *The Middle East in the World Economy, 1800–1914*. New York: Methuen. Pp. xix, 378. \$45.00.

The economic history of the Middle East, which after 1516 is part of Ottoman history, has hardly been studied despite its obvious relationship to European industrialization and expansion. The complex nature of the subject and the multiple linguistic and professional skills required make such a task difficult if not impossible. All this has not daunted Roger Owen, lecturer in recent economic history at Oxford University, from engaging in arduous work—the result of which is this excellent book. In his introductory chapter, which covers three centuries of Ottoman history (1500–1800), Owen begins by questioning the idea of Ottoman “decline” (whether it was absolute or relative vis-à-vis Europe) and then deals (fortunately only briefly) with some theoretical questions, such as the appropriation of the agricultural surplus by officials or notables, the Asian mode of production, and so forth. The following twelve chapters, which include numerous tables and maps, cover in a chronological fashion the economic evolution of the Middle East, that is, of Anatolia (Turkey), Egypt, Iraq, and Greater Syria from 1800 to 1914.

The author regards the economic growth of the

Middle East (the population grew from 11 to 33 million and trade rose from 10 million to 100 million pounds annually) in the nineteenth century as far more complex than other growth models. In effect, Owen claims that changes and growth in the Middle Eastern economy occurred under the impact of domestic forces and through innovation and adaptation rather than solely as a reaction to colonial domination. He points out that the agrarian structure of the Middle East did not consist of isolated units but of interdependent villages and towns locked “into market relations with the wider economy” (p. 42). I believe indeed that the regional and national market linkages of the villages and towns and the vital interdependence between agriculture and crafts—something that needs to be more fully developed—was a critical feature of the traditional Ottoman economy that made town and village highly susceptible to national and international stimuli and influences.

Owen regards trade with Europe as the crucial force that transformed the Middle East economy in the nineteenth century and brought the area under the political, economic, and cultural domination of the West. The abolition of the trade monopoly of the Levant Company in 1825, the subsequent influx of European merchants backed by their governments, improvements in transportation, low tariffs, and accommodating commercial laws, as well as Middle Easterners' own interest in European goods and weapons and cash crops, were some of the chief forces behind the growth of trade with Europe. Owen, though fully aware of the enormous burden resulting from Egypt and the Ottoman empire's growing indebtedness to the West (much of which soon went to pay earlier loans and interest), argues that trade still left a profit margin derived mainly from the agricultural surplus that remained in the hands of a small class of landowners and land controllers.

Owen makes a sincere effort to support his views with data extracted from British Foreign Office reports and published sources as indicated by his very rich bibliography. Nonetheless, his treatment of the nature and inner workings of the traditional Middle Eastern economy leaves many points obscure. Was this economy oriented toward serving socioethical purposes stemming from the nature of the cultural-political system or was it conditioned chiefly or only by economic interests, that is, the sharing of the agricultural surpluses? Were not the cumbersome restrictions and monopolies imposed on internal trade the means for maintaining a steady supply of agricultural commodities in a particular region and at keeping prices at certain levels in order to attain some social goals? If so, then the lifting of the domestic trade restrictions secured by the British in 1838 must be viewed not solely as a technical measure but also as a far-reaching change imposed

on a traditional system in which economics, religion, ethics, and politics were undifferentiated.

Owen does not deal with these problems although he does consider the economic impact of various political and administrative reforms; neither does he make use of the Turkish and Arabic sources though he refers extensively to the writings of Middle Eastern scholars published in Western languages. Moreover, I believe that some of the questions raised by Owen concerning the economic growth of the Middle East in the nineteenth century would have been answered better if he had devoted more attention to the widespread population movements in the area and had made better use of Ottoman statistics. A growing number of scholars agree that the Ottoman statistics, imperfect as they are, appear as the best quantitative measurement of the economic and social changes in the area. In sum, despite a want in native "authenticity," this is a reliable work of solid scholarship with wide conceptual and historical horizons and a major contribution to the economic and social history of the Middle East.

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BARRY RUBIN. *The Arab States and the Palestine Conflict*. (Contemporary Issues in the Middle East.) Syracuse, N.Y.: Syracuse University Press. 1981. Pp. xvii, 298. Cloth \$22.00, paper \$10.95.

Barry Rubin, a fellow at the Georgetown University Center for Strategic and International Studies, has written a concise and lively account of the Arab states' involvement in the Palestine problem. He focuses on the period from the late nineteenth century until the creation of Israel in 1948. Brief chapters on the peacemaking efforts initiated by King Abdullah of Jordan from 1948 until his assassination in 1951 and on Egyptian policy toward the United States and Israel from 1950 to 1956 are appended. (The latter chapter, although providing a fascinating account of the failure of U.S. policy toward Gamal Abdul Nasser, is not organically related to the rest of the book.)

Rubin critiques arguments that Arab foreign policy attempted to distract the public from domestic problems. To the contrary, he believes that the prevailing political culture and belief in the justness of the Palestinian cause seriously constrained the options that could be adopted by Arab rulers. Thus, the Arab rulers could not accept compromise settlements offered by the British or the United Nations since those settlements were unacceptable to the Palestinians, even though the rulers knew that they were unprepared to fight and that they lacked the

geopolitical weight to counter the Zionist movement.

Rubin also analyzes the different perspectives and national interests of the Arab states. Saudi Arabia, Iraq, Transjordan, and Egypt receive most of his attention since they were the only semi-independent political actors in the interwar period. Jealousies among the rulers hampered their ability to develop a coordinated strategy. Those differences were less serious at the time of their intervention in 1936 to end the general strike in Palestine and during the London conference in 1939 than at the time of the UN decision on partition in 1947, when they tried to mobilize to defend the Palestinians. The Arab League, for example, agreed on a military plan only three days before the deadline for Israel to become independent. By then, the land-hungry king of Transjordan had become seriously estranged from the other Arab rulers as well as from the Palestinian nationalists and was determined to occupy part of Palestine in order to expand his own kingdom.

After 1948, the Palestine problem remained "the unfinished business" of Arab nationalism (p. 1). Inter-Arab acrimony over the outcome of the 1948 war has not abated. Moreover, the legitimacy of Arab state intervention in the Palestine problem has not been fundamentally questioned, even though the Palestine Liberation Organization has attempted to assert its military and diplomatic independence from the Arab regimes. Rubin's study, without minimizing the grave disadvantages that the Palestinians and the Arab states faced in relation to the Zionist movement, points out the weaknesses in the way in which the Arabs played the diplomatic game and argues cogently that a more integrated strategic approach might have served them better.

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AMIN SAIKAL. *The Rise and Fall of the Shah*. Princeton: Princeton University Press. 1980. Pp. xiv, 279. \$14.50.

This book seeks to evaluate the shah as a modernizing ruler. Amin Saikal has approached his subject from an "integrative inter-disciplinary point of view" to provide "an in-depth, macro-level analysis of the Shah's rule" (p. xii) by unifying his domestic and foreign policies and linking them to regional and international changes. The sources used are the familiar assortment of published works. There are some surprising omissions. A. K. S. Lambton's *Landlord and Peasant* is cited, not her more relevant book on land reform. The bibliography contains none of Roger Savory's articles.

The historical background of Anglo-Russian rivalry in Persia pictures Russia as motivated by

concern for security while Britain was driven by a fixed determination to expand colonial rule. That Russia occupied northern Persia before World War I with thousands of troops is not mentioned, nor is the bombardment by the Russian Cossack Colonel Liakoff of the Majlis in 1908 or the desecration of the shrine at Meshed in 1912. Readers looking for sound historical background will not find it here. On page 44 we are told: "Mosaddeq was arrested and subsequently sentenced to three years solitary confinement." We are not told that he lived out his life in one of his villages under surveillance, not in prison in solitary confinement.

One major theme is that the shah's government was doomed to failure because of its close association with the West (the United States) in a dependent relationship. The author sees no influences at work in the 1953 coup other than the CIA. Savak is described in the usual terms. We are given the image of a strong, repressive shah instead of the weak and well-meaning man that more accurately reflects his nature.

Chapter 3 contains a useful summary of the White Revolution with the standard criticism of the shah for failure to democratize Iran. The most valuable chapter deals with oil. There the author's account of changes taking place in 1970-71 (led by Qaddafi and the shah) gives these events the revolutionary significance they deserve. He does not overstate when he writes (p. 117) that the Tehran agreement was "historic in its implications" making "a watershed not only in the relationship between OPEC member countries and the companies, but also in oil diplomacy and world politics."

A survey of how the shah viewed his world is followed by an account of Iran's available resources. While progress was made toward the shah's twin goals of "regional cooperation" and combating "communist/subversive" elements, the final collapse was inevitable. It resulted, according to the author, from the shah's own shortcomings and from the inconsistencies of his policies.

This book contains little about the fundamental cleavage, going back centuries, between the dynasty and some of the Shi'i religious teachers.

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AFRICA

ROBERT W. HARMS. *River of Wealth, River of Sorrow: The Central Zaire Basin in the Era of the Slave and Ivory Trade, 1500-1891*. New Haven: Yale University Press. 1981. Pp. xv, 277. \$24.00.

This is a study of the Bobangi and related peoples who live along the Zaire River from Malebo Pool to

the lower Ubangi. The book is divided into three major sections, each of which covers the same four-hundred-year period from a different perspective.

The first part concerns the expansion of commerce that was stimulated by the growth of the Atlantic trade. It includes a discussion of linkages between international, regional, and local economies; the slave trade, both domestic and foreign; ivory production and export; the effect of imports; and commercial institutions such as diplomacy, currency, and language. Of particular interest is the account of the river-borne trade itself, the logistics and economic implications of canoe transportation. This is an important addition to our understanding of Central African trade, since most of the studies done thus far have been of overland enterprises.

Robert W. Harms moves on to a discussion of political and social organization. Early fishing villages were transformed into large, dispersed ethnic alliances that were stimulated by economic opportunities stemming from the Atlantic trade. In the nineteenth century, the process of growth culminated in the establishment of boom towns ruled by trading tycoons and divided into semiautonomous wards. These political units corresponded to commercial firms and their branches. The ward had also become the major social unit and was composed of a wealthy trader, his family, slaves, and clients. Kin groups and blood relations had ceased to be the main focus of social loyalties.

The third section, which is perhaps the most innovative in terms of the argument, considers changing investment strategies and how these affected social structures. Whereas the early fishermen invested heavily in social alliances such as marriage to minimize losses, the traders of the nineteenth century invested heavily in capital and prestige goods to maximize profits. Men came to prefer slave wives, slave children, and slave laborers whom they could fully control. The acquisition of slave children and laborers by the powerful was in part made necessary by the low birth rate in the area in the late nineteenth century. Such fundamental changes caused profound tensions in society. These Harms considers in the context of the Bobangi world view.

Inner Central Africa has almost no written sources before the late nineteenth century, and, as one would expect, this study lacks any precise chronology. Harms does manage to construct a very broad chronological framework in the chapter on international trade, for which the main dates are known. He also attempts to reconstruct the main elements of early fishing societies by extrapolating information from nineteenth-century riverine communities. But much of the discussion remains hypothetical in terms of time, and for this reason the subtitle of the book is a bit misleading. By the same

token, the term "Central Zaire basin" is a bit extravagant since this is mainly a book about the Bobangi, although admittedly the author touches on other neighboring peoples.

There is something for everyone here. Many of the topics that have engaged African historians in recent years are discussed in the Bobangi context, from slavery to disease and from women to ethnicity. This study helps to tie together previously published monographs on West Central African societies that were profoundly affected by the slave and ivory trade that flowed from the Central Zaire basin.

Most importantly, all this is set down with admirable clarity. Harms has collected a mass of fascinating data, but he has not allowed himself to be mesmerized by it. This book is a pleasure to read.

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ASIA AND THE EAST

DENIS TWITCHETT, editor. *The Cambridge History of China*. Volume 3, *Sui and T'ang China*, 589–906, Part 1. New York: Cambridge University Press. 1979. Pp. xx, 850. \$55.00.

For many years scholars interested in the history of imperial China have been awaiting the publication of the volumes of the *Cambridge History of China* edited by Denis Twitchett. This long wait has been amply rewarded. The first of these volumes to appear, volume 3, part 1, is a solid political history of the Sui and T'ang periods (589–906).

The volume is divided into ten chapters: (1) "Introduction" by Denis Twitchett, (2) "The Sui Dynasty" by Arthur Wright, (3) "The Founding of the T'ang: Kao-tsu" by Howard Wechsler, (4) "T'ai-tsung (Reign 626–49)" by Howard Wechsler, (5) "Kao-tsung (Reign 649–83) and the Empress Wu" by Denis Twitchett and Howard Wechsler, (6) "The Reigns of the Empress Wu, Chung-tsung and Jui-tsung (684–712)" by Richard Guisso, (7) "Hsüan-tsung (Reign 712–56)" by Denis Twitchett, (8) "Court and Province in mid- and late T'ang" by Charles Peterson, (9) "Court Politics in Late T'ang Times" by Michael Dalby, and (10) "The End of the T'ang" by Robert Somers.

The introduction is an excellent brief description of the history of this era. In it, Twitchett discusses cultural, economic, and social issues described mostly in passing in the main body of the book, and he successfully sets a context for the largely political events of the succeeding chapters. If someone with little or no knowledge of Chinese history asked me

to recommend a brief introduction to the events and developments of the Sui and T'ang, I would certainly send him to this essay.

Chapter 2 on the Sui is essentially a shorter (and more politically focused) version of materials presented in the late Arthur Wright's *The Sui Dynasty*. Those wanting a fuller picture of Sui developments would do well to turn to the latter book, but the chapter in the *Cambridge History* can certainly serve as a brief introduction.

Chapter 3, on the founding of the T'ang dynasty, is especially worthwhile for the general reader, because Wechsler not only describes the seizure of power and its extension throughout China, but he also deals very briefly with external relations with the eastern Turks and with internal matters such as the formation of the bureaucracy, educational and civil service examination reform, legal codification, and fiscal policies. The chapter closes with a description of the coup that led to the accession of T'ai-tung.

The ensuing chapters generally follow this same pattern. The basic framework is a political history, but at appropriate points there are brief discussions of foreign affairs and of economic, social, and cultural questions. Naturally, the amount of emphasis varies. During the very important reign of T'ai-tung, foreign affairs were of central concern, and so they are dealt with at some length. The chapters dealing with the rise of the Empress Wu and her dominance over the government understandably focus on internal politics. Chapters 8 and 9 focus especially on changes in the distribution of power between court and province and between groups within the court. The final chapter provides an extended description of the gradual erosion of T'ang power and legitimacy.

The depth of coverage of the different reigns varies considerably. One hundred and eighty pages take the reader from the founding of the T'ang in 618 to the accession of Hsüan-tsung almost a century later, while the (admittedly important) forty-four-year reign of that monarch occupies one hundred and thirty pages. The last century and a half of the T'ang fills over three hundred pages. In one sense this uneven pattern is to the reader's advantage. The general reader can find other works in English that describe the political history of the early and middle T'ang but none that treat in any detail the middle decades of the ninth century. The extended treatment of the late T'ang is thus particularly welcome.

Most collaborative works are marred by considerable variations in quality and style; this volume escapes the more serious problems. All of the contributors, as is to be expected, are outstanding specialists in the field, and the judicious hand of the editor has undoubtedly helped to assure a continu-

ity of style. There is, however, one problem of redundancy in chapters 8 and 9, and the reader feels that he is going over the same ground twice. Possibly in the original plan chapter 8 would have been provincial and chapter 9 central in focus. In the end it would seem political history cannot be kept in such neat boxes.

One other, perhaps unavoidable, problem for the general reader will be the plethora of Chinese names. On some pages the reader is bombarded by the names of more than a dozen individuals, and even a reader generally familiar with the subject may find it difficult to remember the relationships among the multitude of individuals mentioned.

These are minor flaws in an excellent book, which will provide a solid background for the companion volume on cultural and institutional history that is now in preparation.

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SUSAN NAQUIN. *Shantung Rebellion: The Wang Lun Uprising of 1774*. New Haven: Yale University Press. 1981. Pp. xvii, 228. \$20.00.

Like her *Millenarian Rebellion in China: The Eight Trigrams Uprising of 1813* (1976), Susan Naquin's new book makes extensive use of the invaluable files of rebel depositions that she discovered in the Ch'ing imperial archives in Taiwan. But this work surpasses that earlier one in its problem-oriented approach. *Shantung Rebellion: The Wang Lun Uprising of 1774* is superbly analytical, rich in detail, and lucidly and forcefully written. It combines realism with understanding and is candid regarding problems that remain unsolved.

The first major internal rebellion of eighteenth-century China, the Wang Lun uprising was regarded by previous historians as a sign of the Ch'ing dynasty's accelerating economic troubles and administrative and military decline. But what actually caused the rebellion to arise in 1774, and why did it occur in that part of Shantung? Having searched the local history sources and carefully considered possible "increase in exploitation, threats to subsistence, violations of accepted reciprocal relations, or relative deprivation" (p. 164), which are factors stressed by social scientists dealing with millenarianism in other cultures, Naquin finds that the uprising did not originate directly from economic causes. The Grand Canal zone of Shantung where it took place was squarely within the commercialized core of the North China macroregion, according to G. William Skinner's regional system. The general populace did suffer periodic dislocation in the prosperous but volatile economy; it seems, however, that

the region was as yet unaffected by the population pressure that was more acute elsewhere in China. Nor was the proverbial misgovernment under the Manchus apparent in the counties where the rebels lived.

Naquin ascribes the sudden rise of this rebellion chiefly to the White Lotus sect's activity itself, but in particular to Wang Lun's reaction to his own successful proselytizing by choosing "to give a new focus to group energies and create an encompassing community through the rebellion" (*ibid.*). Wang, a short, strong man with a pockmarked face, owned some twenty-four acres of farmland but was childless. A country healer, he gained power as master of a White Lotus sect that practiced meditation and martial arts and taught its members that, through dietary regimen and divine blessing, they could become invulnerable to knives and bullets. But what inspired the group to rebel was principally the chiliastic vision inherent in the White Lotus faith, which was only infrequently acted upon. In 1774, Wang declared to his following that the "turning of the kalpa" was at hand and that, to escape terror and destruction, they must respond by an uprising. This enterprise was to take the "Victorious Force" of less than a thousand men and women across three counties, ending in the occupation of the great city of Lin-ch'ing, before they were suppressed and exterminated. Naquin demonstrates that the month-long rebel success was owing to the structural weaknesses of the Ch'ing system, especially the fact that the Green Standard constabulary was deliberately dispersed thinly within each province by the Manchu rulers. But the rebels also earned their temporary triumph. Wang's strategy was ably conceived and his forces did have a command structure, the more orderly because they were not faced with the problem of mobilizing the masses. Unlike other Chinese rebels, Wang did not use egalitarian slogans nor practice "robbing the rich, relieving the poor." Only the sense of the elect's deliverance from the apocalypse runs through Naquin's account of the military drama. The everyday sectarianism of spatially scattered individual members was transformed in battle into community consciousness. "Although we cannot see the millennium they envisioned, we can almost look upon these rebel communities as ends in themselves" (p. 85).

How do we account for the impatient search for the millennium in the first place? For the present, Naquin is satisfied that the chiliastic beliefs were part and parcel of the White Lotus sects' heritage and were "ready at hand." She also theorizes that the tension between Chinese elite and populace may have given rise to a "competitive situation." The elite had the monopoly of government and economic power, as well as sophisticated weapons, "yet elite and masses shared in one culture." The resulting

discontent may well have given rise to the urge to create a rival orthodoxy, with its own "elites, classical texts, and historical vision" (p. 161). The facts of the Wang Lun uprising certainly demonstrate the sectarians' animus against government officials, who were cruelly massacred. Gentry who held high degrees were few in this area of Shantung, although their values must have permeated orthodox popular culture. It is still not clear to me, however, whether the millenarian impulse could be generated by a "competitive situation" alone. To quote Clifford Geertz slightly out of context, "Religion here [as in curing, sorcery, cult groups, and so on] is somehow the center and source of stress, not merely the reflection of stress elsewhere in the society" (*The Interpretation of Culture* [1973], p. 164). Naquin has perspicaciously detected the sectarian desire for "an encompassing community." What remains to be explained is the psychological background of daring and impatience, of the state of mind that made rebelliousness almost an end in itself.

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GILBERT ROZMAN, editor. *The Modernization of China*. New York: Free Press and Collier Macmillan, London. Under the auspices of the Center of International Studies, Princeton University. 1981. Pp. xv, 551. \$22.50.

This collective study from a group at Princeton is a successor to *The Modernization of Japan and Russia* (1975). It is organized along similar lines to make social-science analyses of China's international environment, political structure, economic structure and growth, social integration, and, finally, knowledge and education. Conditions and developments under these categories are analyzed first as "The Heritage of the Past" in the eighteenth and nineteenth centuries and then from 1900 to 1980 as the era of "Transformation." The contributors are highly competent, and their studies under the rubrics of the various disciplines not only compare China before and after the general turning point around 1900 but also offer comparisons with Russia and Japan.

For historians the sections on the late imperial Chinese government by F. W. Mote are particularly rich in insights. They note how China, for example, had a long background of centralized and bureaucratic government that should have made for success in a modern transformation, yet was stymied by a congeries of factors like the alien Manchu rulers' preservation of their power by "surveillance and intimidation" of the Chinese officialdom so that no unified nationalistic leadership could emerge. Un-

like Russia and Japan, China had no tradition of borrowing from abroad. Organs of government between the state level and the local family level were not strong, because the state relied not on village organization but on a local elite based on family lineages.

Trouble in this book arises mainly in summary chapters where "modernization" is treated as a known entity rather than a merely descriptive term. The fact is that "modernization," like its granddaddy "progress," is an imprecise term that can be used operationally only when broken down into components like economic development or political participation that emerge from the assumptions and quantifiable data of a discipline. No amount of social-science underpinning has yet been able to create a framework of ideas that head up in a universally recognizable and quantifiable result known as "modernization." Everyone still has to select his own mixture of indicators of change, and the mixture seems to vary by country as well as by the scholars involved. The forest simply does not emerge from the trees. No doubt this is to be expected when we are all inside the forest.

This frustrating fact leaves the acceptance of the idea of "modernization" to consist partly in faith. When so much growth and change are occurring, we cannot help feeling not only that great processes are at work but also that they can, indeed must, be given an overall name. Our forebears saw "divine providence" at work in history and each of us today may see what she or he will. But such visions are still in the realm of religion and belief, not of verifiable and socially transmissible science. That is in fact the reason why we use the terms modern, modernizing, and modernized so much. They refer to the overall process, the whole stream of change on which we float toward our unknown and problematic future.

The catch is, however, that the social sciences and their indicators do not encompass the whole of our modern experience, especially in the realms of the humanities and of the chances and conjunctures of history (which is not a social science). The record collected under the name of "modernization theory" is inevitably skewed and incomplete. Yet this shortcoming has its useful aspect, for it gives new angles of approach and new perspectives, though not final answers. The emphasis of "modernization" studies is to call attention to similarities and continuities, as when Russia and Japan, the latecomers, are seen to have developed after 1860 comparably strong and effective states in comparable stages, although the content and tone of their thought, aesthetics, and values remained quite disparate.

On this comparative basis China appears in a new light, as even more exceptional than the Sinologist tribe have always liked to think. *The Modernization of China* offers us a new appreciation of how well

China seemed to be prepared for a modern transformation in respect of government (as already noted), of domestic commerce (including contractual relations), a work ethic, a skilled managerial elite, a tradition of technological invention, and the like, all of which only deepens the mystery of China's tardiness and failure to develop like other nations in the nineteenth century and more recently.

One partial key to this complex problem—the sheer size of China's population—seems to me inadequately explored. China's human mass, being so uniquely large, must have set limits on unification, urbanization, transportation, democratic participation, and many other aspects of the modern period. Like most symposia this volume is not up-to-date in its use of the literature of the field and provides only a citation list rather than a systematic bibliography. It does, however, create a solid base for further work, and no one in the China field can neglect it.

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LILLIAN M. LI. *China's Silk Trade: Traditional Industry in the Modern World, 1842–1937*. (Harvard East Asian Monographs, number 97.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1981. Pp. xv, 288. \$15.00.

Was there an important sector of China's modern economy that developed free from the influence of Western imperialism? In a detailed account of how China's traditional silk industry adapted to the opportunities and challenges of the world market during the late nineteenth and early twentieth centuries, Lillian M. Li contends that there was. Her study focuses primarily on the Kiangnan region, the area most involved in the export trade, and secondarily on Kwangtung.

Although the contributions of the silk export trade to China's modern economy were substantial (the value of silk exports nearly offset the costs of cotton imports), Li poses the question that others have asked as well: Could they not have been even more substantial? She maintains throughout the volume that the failure to modernize production and capture a greater share of the world market was a result of commercial practices and institutional arrangements in the silk industry and an economic environment that provided insufficient incentives.

Traditionally, there were great risks involved in the production and marketing of silk, such as the ravages of silkworm disease and the unpredictable fluctuations of the marketplace. Peasant producers minimized these risks by restricting the proportion of their resources devoted to silk production; merchants and industrialists limited their investment in

silk processing and trade. Decentralized networks of household production, manufacturing, and marketing evolved from efforts to distribute risks.

These problems and practices persisted after the opening of international trade. The government failed to initiate new programs to minimize risks and to promote modernization in the industry. There was, for example, no centralized program for the scientific control of silkworm disease as there was in Japan. The government never sought to facilitate credit nor to encourage integration of production and marketing networks. Li argues that problems and practices such as these as well as government inaction slowed modernization and stifled productivity in the silk trade.

Comparing Japan's experience in the silk trade with China's, she concludes that Japan's success and China's failure in the international marketplace were occasioned not, as some scholars have suggested, by Japan's relative freedom from foreign pressures, but by its aggressiveness and dependence on foreign trade as compared to China's self-sufficiency and orientation to the domestic market. She finds no evidence of significant foreign interference in China's silk trade.

It is difficult to quarrel with these conclusions as they apply to the Chinese silk trade. Li has mined an impressive array of source materials and marshaled her evidence well. It would be unwise, however, to extend the inference of imperialist noninterference beyond the boundaries encompassed by her research. Li herself observes that "the silk export trade typified one of the major dilemmas of modern China's history—how to survive a highly competitive international struggle in which it was a reluctant participant" (p. 206)—an acknowledgment that imperialism provided the broad environment in which China's modernization took place.

This sharply focused study of an important sector of China's economy will be of value to Sinologists and economic historians alike.

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JEROME B. GRIEDER. *Intellectuals and the State in Modern China: A Narrative History*. (Transformation of Modern China.) New York: Free Press or Collier Macmillan, London. 1981. Pp. xix, 395. \$22.50.

In the wake of revelations since the Cultural Revolution about the unspeakably harsh treatment to which Chinese intellectuals had been subjected, there has been heightened interest in the historical and contemporary role of that group of men and women who make up the intelligentsia in Chinese society and politics. Jerome B. Grieder's new book has, among other things, the virtue of timeliness. It

covers roughly the period from the middle part of the nineteenth century to the Communist accession to power in 1949, a period of "unprecedented change," as Li Hung-chang has characterized it with prescience. The eight chapters follow a chronological sequence, beginning with a masterful introduction to the intellectual tradition on the eve of the Western incursion into China on all fronts. What follows is the story of China's agonizing response to a challenge for which the decaying empire was totally unprepared.

Woven into this intellectual history is an outline of major historical events providing the necessary perspective in which the impact of the "deepening crisis," to use Liang Ch'i-ch'ao's words, upon China in general and the intellectuals in particular is made more readily understandable. Altogether some sixty or more men of thought and action are included—the only woman is Ch'iu Chin, who made the supreme sacrifice for the revolution—although the weight assigned them varies considerably. As the author admits, selection for coverage is a difficult task, but, given the stated purpose of the book and the criteria set up for inclusion, few would quarrel with the author for his choices. Nor would many eyebrows be raised over the substantially larger space devoted to the life and work of such leading personalities as K'ang Yu-wei, Liang Ch'i-ch'ao, Hu Shih, and Ch'en Tu-hsiu. The careers of Western catalysts—John Fryer, Young John Allen, and W. A. P. Martin among them—form a part of the larger intellectual scene that, despite political turmoil and cultural dislocation, seemed lively and even exciting. There is synchronization between men and events, and the men are presented in "flesh and blood," making this book a good narrative history. The author's own translations from the Chinese sources he quotes has enhanced the book's readability by reducing the "foreignness" of the Chinese language and will prove valuable for holding the attention of nonspecialists.

Even a cursory examination of the footnotes and the bibliography will show how far the study of modern China has progressed in the past twenty years. Grieder has made full and judicious use of recent scholarship in the China field, to which he has contributed with his earlier study of Hu Shih.

As its title indicates, the book is about Chinese intellectuals and the state, and Grieder's central thesis suggests that, under the Confucian orthodoxy of imperial China, intellectual deviation had been kept within stringent bounds. The very exciting story he so succinctly tells here, with all conceivable schools of thought and formulas for political and social action represented, could only happen at a time of precipitous dynastic decline and erosion of the power of the state. The period he deals with deserves to be described as a period of "hundred flowers." Although the story ends before 1949, the

epilogue gives ample food for thought concerning Chinese intellectuals and the state at present and in the foreseeable future.

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STEPHEN S. LARGE. *Organized Workers and Socialist Politics in Interwar Japan*. New York: Cambridge University Press. 1981. Pp. viii, 326.

With this book Stephen S. Large has broken new ground in a field where the basic research on socialist parties has already been done. He focuses on the nexus between the labor unions and socialist parties rather than on the institutional history of the labor union movement. Consequently, although his information, especially on the reform-socialist Nihon Rōdō Sōdōmei (Japanese General Federation of Labor), is interesting and useful, it is his analysis of the union-party interrelationship that demands attention.

Using an approach that he says "transcends" both the "labor relations" approach (stressing internal weaknesses) and the "Taishō democracy" approach (stressing external repression). Large seeks an answer to the question of why the interwar labor movement "erupted" with such "promising vitality, only to sputter and die out" in the 1930s (p. 1). Although he never puts it in these words, his real concern is with the old question phrased by Edwin Reischauer of "What went wrong?" in Japan's modern development during the interwar period and, specifically, why the socialist labor and party movements "failed" to oppose the increasingly authoritarian and aggressive policies of the government during the 1930s. Despite occasional equivocations to the contrary, Large's answer is clearly to blame this failure on the Sōdōmei's dissociation from class-struggle and socialist politics in favor of compromise and "sound unionism" during the 1932–36 period. In passing judgment on this "failure of vision and failure of nerve" (p. 229), Large himself fails to maintain the objectivity that he considers so necessary in the historian (p. 10). But this loss of objectivity cannot be avoided, because of the way he formulates his central question.

Because of his methodological approach, Large merely lays blame but does not explain the depoliticization of the labor movement. If he did not from the outset see the movement as occupying an outsider's position vis-à-vis the mainstream of Japanese society, then the Sōdōmei's refusal to compromise with the Communists in the late 1920s for the sake of proletarian unity would not be surprising. Large persistently expects the Sōdōmei to pursue policies of class confrontation and revolution, even though he points out its acceptance and vigorous

support of fundamental principles and symbols of Japanese nationality from its institutional beginnings (for example, pp. 112–13). Because of this acceptance of the kokutai and emperor system, the Sōdōmei could not be expected to compromise with the Communists. For the same reason, it is doubtful that the Communists would have compromised with the Sōdōmei even if the latter had been willing, especially at a time when the Comintern was identifying social democracy as its chief enemy. The Sōdōmei's rank and file as well as its leaders joined in protest in the 1920s because of economic and political deprivation, but this does not mean that they therefore regarded themselves as outside Japanese society or desired to overthrow the existing society. Large should have given more credence to the words of Sōdōmei leaders who never claimed to be revolutionary socialists and who turned to political action during the early 1920s as a means to effect their essentially economic goals.

Large's commitment to the thesis of the necessity of socialist political action for the labor movement's success limits the depth of his analysis. In chapter 7, for example, he shows that the Sōdōmei's self-imposed retreat from socialist political purpose was not necessary in an objective sense. He concludes that it was "probably premature, albeit perhaps understandable" (p. 167). But rather than going on to explain why it was understandable, Large continues with an adverse comparison between the timidity of Sōdōmei leaders like Matsuoka Komakichi and Nishio Suehiro and the aggressive political mentality of men like Kikukawa Tadao.

Large presents comparative material that may help us understand Japanese developments, but his use of that material is often incomplete. For example, in chapter 6 he quotes Joel Seidman's description of labor unions in general as insecure, short- and narrow-sighted organizations guided by pragmatism rather than philosophy as aptly fitting the Sōdōmei of the 1930s. Nevertheless, in the following chapter he still expresses bewilderment and disappointment that Sōdōmei leaders put the survival of labor institutions above socialist principles and political action. In particular, Large finds fault with them for seeing the favorable change in the employment situation at the end of the depression "as a green light for pursuing nonpolitical sound unionism to strengthen union institutions in and for themselves, not as a positive development that might have strengthened the unions for political struggle" (p. 171). Given the insights gained from Seidman, Large should not have expected otherwise.

Similarly, in the conclusion Large suggests that the Japanese labor movement did not have the opportunity to establish its union institutions as securely as the British and German movements did before engaging in political activism. In other words, "the compressed timing of its history forced

it to try and do many things all at once without having first achieved the capacity to do much of anything well" (p. 240). Large's conceptual framework thus imposes a catch-22 situation on the Japanese labor movement. On the one hand, he doubts that it could have acted politically before it developed institutionally; on the other hand, he criticizes it for giving up politics in order to defend its institutions. Consequently, although Large tells us much of what happened, he still does not tell us why.

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ARTHUR J. MARDER. *Old Friends, New Enemies: The Royal Navy and the Imperial Japanese Navy; Strategic Illusions, 1936–1941*. Oxford: Clarendon Press of Oxford University Press. 1981. Pp. xxxii, 533. \$49.50.

This is the story of the greatest disaster to befall the Royal Navy during World War II. Arthur J. Marder retells it in a uniquely vivid and sophisticated way. He traces the roles the Royal and Imperial Japanese Navies played in their respective governments' decisions for war. He compares their images of one another. In a final section he reconstructs the great battle that brought them together near Singapore on December 10, 1941.

The first section is unavoidably uneven. So at home in British materials, Marder admits that he had difficulty with fragmentary records, translated memoirs, and less than candid interviewees in Tokyo. Yet by juxtaposing the two navies he succeeds, in ways other historians have not, in explaining how increasingly limited the admirals' choices and influence were. Within the Admiralty senior officers did not plan for a three-enemy war. They disagreed over the probable *casus belli* and failed to win sufficient American naval support. But the real responsibility was Winston Churchill's. Drawing a false analogy between what Hitler's *Tirpitz* had done in the Atlantic and what the *Prince of Wales* and *Repulse* might do in the Pacific, he sent the Royal Navy's newest capital ship toward disaster. In Tokyo, naval officers had neither will nor ability to turn military and civilian leaders away from war with Britain. Carefully sketching roles and personalities, Marder explains more fully than Stephen Pelz (1974) or Hosoya Chihiro (1980) why Imperial Japanese Navy leaders in September 1940 approved alliance with Germany and ultimately chose battle with Britain and America rather than conflict with the Imperial Japanese Army.

Marder's comparison of the two navies is insightful, but his elucidation of their images of one another is both fascinating and frustrating. While drawing a fuller picture of the education and ethos

of the Japanese naval officer corps than any other English-language historian, he regrettably accepts clichés advanced by Britons in the 1930s and Japanese ever since. He reveals how British naval officers' prejudices led to their underestimation of the prospective Japanese foe. Yet despite the thoroughness of his explanations of the particular, one comes away wishing that he had reached out beyond the navy to relate those prejudices to their taproots in Western governmental elites and societies more generally.

Marder is at his best in describing and analyzing the naval battle of Singapore itself. His account is fuller and livelier than those of S. W. Kirby (1971), Stephen Roskill (1976), and Raymond Callahan (1977), because he uses Japanese sources and reconstructs events through the eyes of participants. In so doing, he makes even the hapless British commander, Vice Admiral Tom Phillips, a credible hero. Placed in an impossible position, he chose risk over retreat, sortieing without air cover in hopes of surprising the Japanese forces attacking British troops. That he met death was due to bad luck, arrogant refusal to believe that torpedo aircraft were nearby, and faulty maneuvering once they struck. Marder sketches a bittersweet portrait of the Japanese victors. Admiral Kondō Nobutake regretted that his surface ships had no part in the action, and his aviators scarcely believed they had sunk the Royal Navy's finest ship.

This final volume by one of the greatest naval historians of the century cannot compare with his *From the Dreadnought to Scapa Flow*, yet no naval historian, indeed no one interested in the historian's craft, can afford to miss it. In its breadth of conception, research techniques, organization, and even in its pointed disagreements with other historians, it brings out the best in its author and in "traditional" narrative history. That this genre is not dead, and that it can illuminate the past in a uniquely meaningful way, become unmistakably clear in this work of a master craftsman.

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JAMES NEIDPATH. *The Singapore Naval Base and the Defence of Britain's Eastern Empire, 1919–1941*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xvii, 296. \$59.00.

The tangled and ultimately tragic story of British naval policy toward Japan prior to the outbreak of the Pacific war continues to attract historians. Within the past two years important studies have appeared by W. David McIntyre, Paul Haggie, and the late Arthur Marder. Now a fourth work has materialized that provides a careful and lucid investigation of the development of the Singapore naval base and

the controversies surrounding it. James Neidpath relies principally on the numerous records in the Cabinet Office and Admiralty archives in the Public Record Office together with relevant private papers; apparently little use has been made of the unpublished Foreign Office records, however.

At the end of the First World War Britain was faced with the problem of devising a viable defense policy for the Far East and Pacific, given the fact that the German threat had been eliminated and that the Anglo-Japanese alliance was unlikely to continue for much longer (the alliance ended formally in 1923). It was held by ministers and naval chiefs that the most appropriate policy was to construct a major base at Singapore, which could meet the requirements of a fleet sent from the Mediterranean in the event of trouble arising with Japan. Since Britain faced no European foes in the 1920s and the "ten year rule" gave official recognition to the improbability of conflict in Europe, this policy might be regarded as justifiable in the 1920s. Neidpath, in one of the most interesting aspects of his monograph, brings out the extent of contemporary doubt in the 1920s as to whether Britain ever would take the risk of dispatching the fleet to the Far East. Cogent criticisms in this direction were voiced by the prominent Liberal MP George Lambert, by General Smuts, and by Admirals Scott and Kerr. These warnings accurately foreshadowed the events of the 1930s when the Singapore strategy was revealed as a bogus policy.

A recurring theme in the attitudes of the British governments of the period is the preoccupation with the Dominions and the belief that Britain had to provide reassurance that imperial interests would be safeguarded. Hence the increasingly unrealistic promises extended by British prime ministers, culminating with Churchill's undertakings to Australia in 1940–41. Fundamental to the British position was the grave underestimation of Japan, itself determined in part by a mixture of ignorance and racial prejudice. In addition, domestic political constraints within the United States precluded the development of full Anglo-American cooperation that, as the author emphasizes, constituted the only effective means of opposing Japan. This study is most useful for providing a clear guide through the complexities of Britain's Far Eastern defense planning in the interwar years.

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J. T. F. JORDENS. *Swāmī Shraddhānanda: His Life and Causes*. New York: Oxford University Press. 1981. Pp. xv, 210. \$24.50.

One of the most consistent problems in South Asian history has been a lack of scholarly biographies,

particularly biographies that are reliable, sophisticated, and not merely panegyric. J. T. F. Jordens is clearly attempting to fill this void. He has completed his second scholarly biography, this time of Swami Shraddhananda, an important figure in late nineteenth- and early twentieth-century India. Swami Shraddhananda was known by his original name, Lala Munshi Ram, until 1917 when he took the vows of a Hindu holy man or *samnyasi*. The swami became one of the major leaders in the militant wing of the Arya Samaj, a reformist and revivalist Hindu movement in northwestern India. Shraddhananda also participated in the nationalist agitation that followed World War I and in the first noncooperation campaign led by Mahatma Gandhi. After the collapse of this round of political conflict Shraddhananda led one of the struggles to raise the status of Hindu untouchables; he also led the highly controversial campaign to reconvert Muslims to Hinduism. For a period of four decades, from 1883 to his assassination in 1926, Swami Shraddhananda was continuously involved in religious and political movements.

The swami was a man of temperamental extremes. At times he was passionately dedicated, highly disciplined, and effective as a leader. Yet there were periods of his life when he lacked direction and stability. Also, because of his own rigidity, he found it difficult to succeed as a leader. He could not compromise on an issue and could not understand those who did not share his own extreme vision. Jordens accurately characterizes the swami "as a man inspired by ideals and sometimes impossible dreams in the midst of his more pragmatic colleagues and collaborators" (p. xv).

This volume by Jordens is an excellent example of biographical scholarship. He bases the study on extensive research, in which he used sources in English, Hindi, and Urdu that he collected through his travels in India and England. Among this material is a wide variety of historical sources: books, pamphlets, journals, newspapers, organizational reports, memoirs, and government documents. Using these sources skillfully, the author places Swami Shraddhananda in terms of his own personal development and in the cultural, intellectual, and general historic context in which he lived. As a result he has produced a volume that will be valuable for students and scholars. Those individuals interested in modern Indian history and those focusing on developments within Hinduism will find this study directly relevant; scholars concerned with cultural and intellectual changes within a colonial framework can also benefit from this volume. There is no question that Jordens has once again contributed significantly to the scholarly literature on modern South Asian history.

KENNETH W. JONES
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LAU FONG MAK. *The Sociology of Secret Societies: A Study of Chinese Secret Societies in Singapore and Peninsular Malaysia*. (East Asian Social Science Monographs.) New York: Oxford University Press, 1981. Pp. ix, 178. \$29.95.

Secret societies have historically played an important role in many Chinese communities and have long fascinated Western observers. A rich literature describes the history and activities of Chinese secret societies in China, Hong Kong, and Southeast Asia. This book by Lau Fong Mak, a Singaporean Chinese scholar, differs from most of the earlier studies in that the author's interests are frankly sociological and theoretical rather than descriptive and narrative. Mak makes good use of primary materials, especially interviews and an examination of Chinese street names, but much of his analysis utilizes data from the extensive published record. The book inquires into how and why secret societies developed and flourished among the Chinese in Malaya and Singapore from the early nineteenth century to the present.

Mak devotes most of his book to demonstrating how the emergence and persistence of Chinese secret societies have been affected by three factors: the inadequacy of legal protection for Chinese settlers in a multiethnic society; the adaptability of secret societies to changing conditions; and the strength of the societies in supplying conflict-reduction mechanisms. Although he refers to other communities, especially in Perak, most of his supporting material derives from the experience in Singapore, Penang, and Malacca. On the whole Mak's argument and theoretical thrust seem reasonable, and his observations on recent developments are particularly interesting: he demonstrates, for example, how secret-society activity since 1969 has declined to a low or moderate level due to increased legal access for Chinese residents. At the same time he believes that secret societies will continue to exist in the future, because the conditions that encourage their activity still persist.

Although by no means comprehensive, Mak's book is a valuable addition to the field and can be used advantageously with the earlier historical accounts. The serious student of Chinese secret societies will learn little that is strikingly new, but the book contains much useful information on the Malaysian Chinese. Inevitably, the book contains problems and deficiencies. The author's turgid prose, heavily laden with sociological jargon, renders the material difficult for nonsociologists. Mak occasionally mentions comparative cases of "underworld" organizations; his argument would have been enhanced by extending this part of the analysis. He uses terms like "modernization" uncritically and also fails to differentiate between urban- and rural-based secret societies. Mak's analysis also un-

derplays both national politics (such as the rise of a Malay-dominated government in Malaysia) and broad economic patterns. Sometimes his data seem incorrect; for example, he confuses the multiethnic and multidialect town of Pontianak with the largely Hakka mining camps in the hinterland of West Borneo. The author also ignores some comparative data that challenge his argument: if indirect rule of the Chinese in a colony led to secret-society formation, why did these organizations not appear in towns like Kuching? Mak's book is an informative one and well argued, but it may not be the last word.

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WILLIAM J. DUIKER. *The Communist Road to Power in Vietnam*. (Westview Special Studies on South and Southeast Asia.) Boulder, Colo.: Westview Press. 1981. Pp. xvi, 393. \$32.50.

In introducing his *The Communist Road to Power in Vietnam*, William J. Duiker states that "... to avoid superficiality, I have restricted my concern to one of the central issues raised by the conflict—the nature of the Party's revolutionary strategy toward the seizure of power." This book is thus not a comprehensive history of the war or of the Communist movement per se, and these restrictions contribute both to the strength and weakness of Duiker's study.

Beginning his book with the rise of Vietnam's revolutionary movement around 1900, he concludes it by reviewing postvictory developments and by evaluating the factors in the Communists' success. Duiker is strongest in describing and analyzing the party's decision-making processes and in following the twists and turns in policy and strategy. Often the only sources available are Communist documents published in Hanoi or captured by its enemies. Duiker indicates his awareness that some of these documents were propaganda or were deliberately designed to be misleading. From these documents he has pieced together a detailed narrative showing the flexibility with which the North Vietnamese Politburo and its cohorts in South Vietnam adapted their strategy and tactics to changing circumstances and met new challenges ably, although sometimes only after agonizing reappraisals.

Duiker has given little attention to the significant roles that propaganda and indoctrination played in the party's successes in both the north and the south. He underplays the important pattern of political assassinations and terrorism that accompanied the Communists' rise to power. Unmentioned is their betrayal to the French police of nationalist youths who resisted Communist proselytizing while attending the Kuomintang's Whampoa Political and Mili-

tary Academy in Canton. Ignored are incidents like the murder of Ngo Dinh Diem's older brother and the imprisonment of Diem in 1945, which contributed to Diem's bitter anti-Communist stance when he became president of South Vietnam's government. For a more critical view of such tactics one must still turn to studies like Robert F. Turner's *Vietnamese Communism: Its Origins and Development* (1975).

Overreliance on Communist sources may have led Duiker to be less critical of Communist actions than of the actions of their opponents. For example, in discussing Diem's anti-Communist drive beginning in 1954, Duiker cites the party's casualty figures (p. 174), but, in writing of the subsequent assassination of hundreds of local officials by the Communists, Duiker gives no figures but quotes the party's terminology referring to the "punishment" or "elimination" of "corrupt officials" or "traitors."

Such omissions or underplaying of information may be due less to intentional bias than to the trap set for all writers using Communist documents. Communist jargon is insidious. It is difficult to quote or even paraphrase party documents without sounding propagandistic. Thus, Duiker writes of "patriotic elements" when referring to persons favorable to the National Liberation Front, but he labels its opponents "traitors" (p. 197).

Overall, Duiker has done fine work in combing through these mind-numbing sources to produce, within its limitations, a useful history of the Vietnamese party's rise and its final victory. From it emerges the image of a highly dedicated group of Vietnamese Communists, led by Ho Chi Minh until his death, who were determined at any cost to create a united and independent Communist Vietnam. In pursuing that goal they sometimes acted against the advice of their chief sources of aid, the Soviets and the Chinese Communists, while at the same time cleverly manipulating both to continue their aid despite the bitterness of the Sino-Soviet dispute.

CHESTER A. BAIN
Laguna Hills, California

SAMUEL P. HUNTINGTON. *American Politics: The Promise of Disharmony*. Cambridge: Belknap Press of Harvard University Press. 1981. Pp. viii, 303. \$15.00.

Samuel P. Huntington's scholarship includes writings on political participation in developing countries, on democratic crisis, as well as his *Political Power: USA/USSR* that he published with Zbigniew Brzezinski in 1964. Now, in his latest and widely announced work, Huntington attempts to blend

intellectual history with his more pragmatic and accustomed interests. Although its publisher acclaims *American Politics: The Promise of Disharmony* as ranking beside the work of Tocqueville and Lord Bryce, most historians are likely to find it disappointing. The book has many strengths, certainly, but these are mostly confined to Huntington's discussion of national disunity during what he calls "The S & S Years," by which he refers to the period 1965–75, and to his concluding chapter that speculates upon "The Viability of American Ideals and Institutions." These two chapters of a contemporary and futurist character, comprising nearly half of the book, find the author comfortably at home.

The earlier portion, however, is a strained attempt to systematize the course of American nationalist thinking from the Revolution through the era of the New Deal, evidently without exploring the massive sources where an understanding of this subject rests. Nor has Huntington read many of the books written recently by historians of American ideology. At least there is nothing in his narrative or notes to indicate such a foundation. The book has no bibliography. More significant is the absence of American nationality as a complex and seamless feature of American thought. Historians now recognize that such blendings of event and concept as mission, the Founding Fathers, union, nativism, the Civil War, and expansionism were enduring and resisted being confined to a limited time. Instead, they required that the public await national fulfillment in some degree of realism, for America's national self-consciousness has been mostly a spirit of anticipation.

Huntington prefers a view of American awareness that is segmented in periodic national responses to the "I v I gap," his term for the discrepancy between America's ideals and attainments. The result, he says, is a "disharmonic polity" that has caused four outbursts of "creedal passion." These are the American Revolution, the Jacksonian era, the Progressive age, and the outrage of the 1960s and early 1970s. All represented times when Americans tried to reduce or eliminate the "I v I gap" by moralistic efforts at reform. From such eras, it is argued, came a refreshed American reverence for its "creed," comprised, the author says, of liberty, equality, individualism, democracy, and the rule of law under a constitution. By living in hope that we will continue to have these rejuvenations periodically, Huntington believes there is a chance that America will not surrender to complacency, stagnation, cynicism, and hypocrisy, the dangers that he contends accompany the nation's awareness of the "I v I gap."

PAUL C. NAGEL
Virginia Historical Society

GWENDOLYN WRIGHT, *Building the Dream: A Social History of Housing in America*. New York: Pantheon, 1981. Pp. xix, 329. \$18.50.

The home is a terribly attractive subject for social historians, and it has accordingly called forth an enormous literature. Because almost everyone has some sort of abode and because these shelters invariably leave some sort of trace on the ground and in legal records, the dwelling tells the historian something about even anonymous people like slaves, farm laborers, and serving girls. There is even now some undiscovered tax list, some new cellar hole, or highway cut that will bring to light fresh evidence about Americans' past domestic arrangements.

Not only does evidence abound but also the home itself offers a wonderful variety of dimensions. It may be studied for its technical components as David Handlin has done, or for its domestic implications as in Dolores Hayden's research, or it may be examined according to the canons of architectural iconography as Vincent Scully does, or it may be interpreted by manipulating numerical aggregates as Theodore Hershberg and his fellow Philadelphia data-bank scholars do. Such a topic, such a vast literature, offers the summarizer a tempting field and a useful opportunity.

Gwendolyn Wright has taken up this task in her little book, which combines in easy summary form a quick survey of the major points of recent American social history and recent studies of women and the family with the history of some major house types. The thirteen summary chapters give the social history contemporary to the structures. The chapters and subjects are: seventeenth-century New England; early nineteenth-century romantic-classical urban row houses, slave housing, factory boarding houses, and rural cottages; Victorian suburbs, urban slums, apartment houses, and apartment hotels; and twentieth-century bungalows, company towns, private planning and zoning, public housing, and post-World War II suburbs. The illustrations are few and minimal, but the writing is clear and the author's summary easy enough to follow. A useful bibliography for the topics of each chapter is appended.

I think of the book as an elementary text for students of American urban, social, or architectural history. If the professor is willing to provide the theory and the historical interpretation, it could well help the beginning student to get over a lot of ground. The historian should be forewarned, however, that there is no explanatory theory in this book, nothing to help one understand the varying paces and varying logics of changes in family life, building technology, housing or land demand, or iconography. This is merely a survey of things that

are contemporary with one another and thus there is no intellectual advance here over the old twelve-volume *History of American Life* to which it might be regarded as a partial update.

SAM BASS WARNER, JR.
Boston University

JAMES G. MOSELEY. *A Cultural History of Religion in America*. (Contributions to the Study of Religion, number 2.) Westport, Conn.: Greenwood Press. 1981. Pp. xviii, 183. \$25.00.

In nine depictions of as many movements in the history of American religion James G. Moseley chose nine current interpretive motifs to provide special insight. The history of religions refracts the Puritans, psychology of religion provides the lens for the Great Awakening, politics is the window on revolutionary religion, through literary criticism is seen transcendentalism, ethics opens on the Gilded Age, theology gives a view of the impact of neo-orthodoxy, modernization theory interprets the socialization of religion in the twentieth century, and the notion of "revitalization movements" grants a vista of contemporary American religion. The preface and epilogue, each quite terse, claim that "each generation of Americans has produced a new and vital way of being religious." Culture is taken to be "the web of meanings through which humans have personal and social being"; religion is "the way people symbolically express their culture's relation to a primordial, fundamental order or ground of reality" (p. 161).

The episodes, presented chronologically, are hardly apposite. Chapter 4 is on "religious freedom, revivalism, and the churches: a sociological approach to the second great awakening" (p. 51). Chapter 9 is on "the two Niebuhrs: a modern theological renaissance" (p. 105). A more accurate title for the work might have been, "themes in (or on) the relation of religion to American life." Certainly it falls far short of instructing us as to how "each generation" found its "way of being religious."

The borrowing of interpretive categories from one field of study to shed light on topics in another accounts for such a vast range of recent writings in history (to say nothing of the rest of the humanities and social sciences) that a single author can hardly be faulted for joining the crowd. Surely there can be no doubt that wearing eyeglasses fitted to somebody else gives to whatever one looks upon a different appearance, and familiar episodes in American religion are no exception. But to choose different specs for each of the nine episodes outdoes the trend. The result might have been dizziness, but for the fact that this book provides glimpses and glances rather

than inspections or examinations. For example, the Civil War's bearing on this study is that it, along with the failure of Reconstruction, made Emerson's religiousness "appear simplistic" and showed the need of the Gilded Age for a "new way of being religious" (p. 81).

Moseley writes tersely, precisely, and fluently, and this writing lends the book a certain charm. Each brief chapter holds its own peculiar interest. The bibliographical essay takes the broad view and includes very recent works.

WILLIAM A. CLEBSCH
Stanford University

J. M. SOSIN. *English America and the Restoration Monarchy of Charles II: Transatlantic Politics, Commerce, and Kinship*. Lincoln: University of Nebraska Press. 1980. Pp. 389. \$25.00.

For the first time since the days of Osgood, Beer, and Andrews, two important large-scale interpretations of Anglo-American relations in the late seventeenth century have recently appeared. They present diametrically opposite views of Restoration colonial policy. Stephen Saunders Webb, in *The Governors-General: The English Army and the Definition of the Empire, 1563-1681* (1979), argues that Charles II and his advisers pursued a purposeful policy of military control, later elaborated by James II and William III. According to Webb, Charles II's paramilitary garrison governments gave definition to the Anglo-American system, and the constitutional settlement forged in Jamaica and Virginia in 1681 "became the fundamental law of empire" until 1783. J. M. Sosin sees matters altogether differently. He argues that relations between the home government and the colonies, 1660-85, were characterized by inertia, ignorance, and incompetence. Sosin challenges the traditional argument of Charles McLean Andrews that the English government gradually evolved a systematic commercial and colonial policy during these years. Instead, he asserts, "the government of Charles II had no overall plan; it reacted to disparate events as they occurred in the individual American colonies" (p. 181). Far from erecting Webb's network of coercive garrison governments, the king's ministers were always trying to reduce colonial expenses. In 1679 the royal treasury allocated the meager total of £17,517 for the civil and military establishments in the five royal colonies, while collecting about £125,000 in tobacco and sugar taxes from these colonies. Sosin pictures most royal agents—men such as Maverick, Carr, Randolph, Culpeper, and Cranfield—as foolish or corrupt or both. He finds little difference between Charles II's liberal bestowal of colonial charters in

the 1660s and the royal campaign against charters in the 1680s. And he finds no major constitutional settlement in 1681.

Who is correct—Sosin or Webb? In my judgment, Sosin has the better argument, Webb makes the better presentation, and both men overstate their positions. Webb is surely correct in pointing to the rising importance of military governors, but he cannot make a convincing case for garrison government in 1660–81 because the army under Charles II was small and feeble, and the officers sent to administer the royal colonies during his reign were generally self-serving opportunists, not imperial statesmen. And Sosin pushes too far in the opposite direction. While he does a splendid job of demonstrating the Stuarts' casual attitude toward America, he ignores clear evidence of efforts toward centralized colonial planning and authoritarian royal rule, especially in 1675–85.

There is a second side to Sosin's argument. He complains—very justly—that most historians of seventeenth-century America dwell myopically on local events, as though each colony was autonomous and independent, and so he sets out to establish the importance of transatlantic ties of kinship, trade, and culture. Yet after the opening chapters he abandons this theme and reverts to a conventional political narrative that mainly recapitulates familiar events. Sosin has conducted very extensive archival research, principally in the Colonial Office papers, but he sometimes loses the reader in a morass of archival detail, and he has the bad habit of hiding his most interesting analytical commentary in the footnotes. Furthermore, he has fallen victim to another form of historical myopia very common among U.S. specialists on colonial America: he excludes the island colonies from his definition of "English America." Here Sosin most clearly departs from his evidence, for if the Colonial Office files reveal nothing else, they demonstrate that Barbados, Jamaica, and the Leeward Islands occupied a central position in the Anglo-American web of politics, commerce, and kinship in 1660–85.

RICHARD S. DUNN
University of Pennsylvania

LARRY DALE GRAGG, *Migration in Early America: The Virginia Quaker Experience*. (Studies in American History and Culture, number 13.) Ann Arbor: UMI Research Press. 1980. Pp. viii, 135.

Quakers and colonial Pennsylvania, to paraphrase Sammy Cahn, "go together like a horse and carriage." But do they? Not entirely, according to Larry Dale Gragg in this study of Quakers in eighteenth-century Virginia. In fact, as the author shows, there

were Quaker congregations in the Old Dominion before William Penn established his holy experiment. The thrust of this little book is not, however, an accounting of how the Friends prospered or failed in Virginia's more hostile social climate; instead, it aims at describing their migratory peregrinations.

Gragg's analysis relies upon facts gleaned from a reconstitution of the lives of 2,445 Quakers for whom certificates of removal survive in the records of the monthly meetings. "From this data [*sic*]," Gragg maintains, "it will be possible to examine, most notably, the age, family status, distance moved, and frequency of movement among the Quakers moving into, within, and out of Virginia between 1711 and 1800. Having done this it will then be possible to compare this particular group of colonial American migrants with their modern counterparts and to test the universality of migration differentials" (pp. 7–8). He does all of this with a vengeance, principally by illustrating these characteristics through a series of tables. These tables constitute 32 pages, or 36 percent, of the entire text, with much of the remainder being used to enlarge upon the tabular evidence: a "typical" migrant was a young male, who had a family and moved 1.86 times in his life. He often traveled from Pennsylvania to the Valley of Virginia, from whence he moved south, or, in local parlance, "up" into the Carolinas, and thence westward towards the Ohio country. His reasons for moving were partly economic and partly associated with Indian troubles, war, slavery, family attachments, religious convictions, and just plain restlessness. In short, they differed little from other colonials or modern Americans.

Readers will admire the diligence and energy that went into the preparation of the tables, but the more discriminating of them will wonder if the product was commensurate with the labor. The tables, together with the text, merely document the obvious, the trivial, or the meaningless. In fact, this book is an almost perfect example of how sophisticated methods sometimes go awry. Methods are only as good as the queries that inform them, and in this work Gragg has not asked especially useful questions. The book is afflicted, moreover, by the pedestrian prose style and ghastly usage that can curse authors who employ quantitative methodologies. Must the historian's lexicon be infested with neologisms such as "in-migrants" and "out-migrants" when "immigrants" and "emigrants" convey the same meaning, but with greater felicity?

There is need for studies of Virginia Quakers. From the 1650s to the early 1800s they were an important element in Virginia's population. Sad to report, this book does not contribute to an understanding of their part. Indeed, the discerning schol-

ar will avoid the ground Gragg has charted. It leads to a cul-de-sac.

WARREN M. BILLINGS
University of New Orleans

RICHARD F. LOVELACE. *The American Pietism of Cotton Mather: Origins of American Evangelicalism*. Grand Rapids, Mich.: Christian University Press; distributed by William B. Eerdmans, Grand Rapids. 1979. Pp. x, 350. \$9.95.

The decade of the seventies was—at long last—Cotton Mather's decade. After years of dragging around the rattling tin can that his critics had tied to his tail, Mather finally garnered a measure of respect. Such historians as Kenneth Silverman, Robert Middlekauff, and David Levin concluded that he was neither a simpering fool nor a simplistic bigot but rather a complex figure who embodied the ambitions and tensions of the later New England Puritan tradition. Now Richard F. Lovelace has tried to complete the revised portrait. By locating Mather within ancient and modern traditions of spirituality, he tries to depict him not only as a man of coherent and self-consistent character but also as an early revivalist whose accomplishments exhibit the Puritan origins of the Great Awakening.

For Lovelace, Mather was the most important American writer of his generation on topics of spirituality and religious experience, and he believes that Mather's theology of Christian experience provides the clue for understanding the seeming inconsistencies and idiosyncrasies of his character. Viewed from within the heritage of patristic and medieval asceticism, Mather appears as "merely an ordinary contemplative ascetic with literary ambitions and a bent for activism" (p. 285). So viewed, moreover, he emerges not as a harbinger of Enlightenment rationalism but as a chastened Reformed orthodox theologian, whose works reflected the interests in practical piety and ecumenism that marked the late seventeenth century.

Most of the book is a careful, thoughtful reconstruction of Mather's theology—his notions of revelation, regeneration, justification, sanctification, Christ, and the millennium—and his methods of piety and evangelism. Despite the book's title, Lovelace does not argue that Mather bore any special indebtedness to the continental Pietists. He does believe, though, that they helped to arouse Mather's interest in social reform, which led him toward a new strategy of mission in which the church would leaven the social order by witness and action.

Lovelace nicely clarifies the broader setting of spirituality that intrigued Mather. He also writes with a sound knowledge of sixteenth-century Reformed theology. Indeed, the book is so thoroughly

characterized by balance and sobriety that it seems to hide as well as reveal its main character. Lovelace recognizes that Mather had some problems; he dissects the notorious overblown ego; but he is so intent on viewing him as the embodiment of a tradition that he attributes most of the eccentricities to a few flaws and misplaced emphases in traditional Puritan theology, notably an imperfect balancing of the themes of law and gospel, justification and sanctification, and faith and introspection. But few other Puritans writhed and whined as much as Mather, even though they accepted the same religious doctrines. Lovelace has written a generous and knowledgeable study of Mather's thought; he has not sustained the claim that the theology is "the key to his character and achievements" (p. 3).

E. BROOKS HOLIFIELD
Emory University

NORMAN FIERING. *Jonathan Edwards's Moral Thought and Its British Context*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1981. Pp. xiii, 391. \$30.00.

This book is a sequel to Norman Fiering's distinguished *Moral Philosophy at Seventeenth-Century Harvard* (1981). With the decline of scriptural authority among intellectuals, serious talk about man and God became the property of moral philosophers who, between 1650 and 1750, undertook to re-establish morality on a new foundation with no direct dependence on traditional religion. The feat was accomplished and, as Fiering the moralist observes, served civilization well until recently. But Judaeo-Christian values were deeply imbedded in the new secular ethics, and Jonathan Edwards was challenged, as an avowed Christian, to show that these values were fundamental and could be defended with the same rational methods, without recourse to revelation, that the secular philosophers were using.

Edwards read the new moralists and in a series of books adapted their arguments for a penetrating defense of Christian ethics. Central to Edwards's thought on ethics were his conceptions of original sin, will, and hell. The Christian anthropologist had to prove, without referring to Eve and the apple, that mankind prefers evil to good. To do this, Edwards made use of the latest psychological investigations of secularists like Hobbes. Edwards failed to shake the faith of his age in the goodness of man; it took two great wars and the Holocaust to do that.

Edwards had also to defend, as did Augustine, Aquinas, and a host of Protestant scholastics, the proposition that divine sovereignty leads inexorably to the doctrine of predestination but that predestination does not vitiate man's moral accountability.

Edwards, like his predecessors, did not solve this paradox, but, Fiering says, he did contribute to a clarification of the philosophical problems involved.

Finally, Edwards took upon himself the formidable task of justifying, with philosophical arguments, the necessity of the eternal torments of hell, and here, Fiering says, he was more successful. He did make a kind of sense of the proposition that the blessed in heaven, contemplating the misery of the damned and considering that their misery will endure forever, will quite properly rejoice exceedingly. Fiering's description of Edwards's thoughts on hell is the best part of his book.

Fiering is contemptuous of Perry Miller's work on Edwards and certainly much good new work has been done since Miller wrote. But Fiering ought to recognize that he is following in a tradition of sympathetic and learned engagement with Protestant scholasticism in New England of which Miller, if not the originator, was certainly the most distinguished representative of his generation.

Fiering does no more than mention in a phrase or two Edwards's own psychological nature. Edwards had an obsessional fear of hell that was connected with his own overwhelming feelings of guilt. He clung to the doctrine of hell, after some initial doubts, with the fanaticism of a new convert. If psychobiography has any validity, then Edwards is surely a prime subject.

This book is a skillful and subtle analysis of an important intellectual tradition and, as a moral philosopher himself, Fiering suggests that our morally muddled age may learn something from the story.

RICHARD SCHLATTER
Rutgers University

KENNETH A. LOCKRIDGE, *Settlement and Unsettlement in Early America: The Crisis of Political Legitimacy before the Revolution*. (Joanne Goodman Lecture Series.) New York: Cambridge University Press. 1981. Pp. ix, 134. \$9.95.

In this short book Kenneth A. Lockridge sketches an interpretation of the history of political and social legitimacy in colonial America. Focusing conventionally on Massachusetts and Virginia, he argues that the disparate colonies shared a common crisis of legitimacy, Massachusetts moving from a delicately ordered system to a conflict between the principles of hierarchy and "evangelical localism" and Virginia moving from chaos to the same conflict. In New England initial order stemmed from the unanimity with which Puritan settlers sought to replace the greed, individualism, and disorder they perceived in England with love, communal values, and submission to God's will. The classic Puritan value of

moderation created a momentary balance between the leveling tendencies of communitarianism and the hierarchical pretensions of would-be aristocrats, but it was an inherently unstable balance. By 1700 synthesis had degenerated into bare assertion of the traditional principle of centralized hierarchy by the elite and a grudging, "visceral localism" on the part of local communities, which continued an age-old English resistance to the claims of centralization. Once begun, the struggle of legitimacies continued down into the Revolutionary era.

Meanwhile Virginia had begun its history in a chaos of turbulence and insecurity derived from absence of agreement on statuses and legitimacy and reflected in the familiar history of starving Jamestown and Bacon's Rebellion. Through the period of chaos, self-conscious gentlemen strove to build legitimacy by manipulating the conventional trappings of social hierarchy and in the aftermath of 1676 achieved at least the surface success reflected in the eighteenth-century aristocracy. But Virginia aristocracy remained too self-conscious and "brittle" to bespeak real legitimacy, and it continued to coexist with a backcountry localism that sought merely to be left alone. When the Great Awakening provided this localism with evangelical underpinnings, the "cultural opposition" already manifest in New England became overt in Virginia as well. The clash of hierarchy and localism informed the social and political conflicts of the Revolutionary era until both principles, unable to triumph over one another, were transformed by and subordinated to a democratic individualism that emerged from revolution and found expression in Madison's Federalist No. 10.

Necessarily sketchy in its historical narration, a published lecture series such as *Settlement and Unsettlement* must be judged on its ability to clarify the thematic issues it raises. Seen in this way, the book enjoys substantial, although limited, success. Lockridge successfully draws together three themes that have been conspicuous in early American social history: the maintenance (some say growth) of European social patterns, the persistence of a communitarian localism, and the emergence of individualism. By integrating these themes with large-scale political change, he outlines one interpretation that would merge social history with traditional history, and with a traditional emphasis on the Revolution. Given the propensity of social history to separate itself from traditional interpretations, that is no mean achievement. But many of the specific themes of the book remain sketched rather than worked out. Whether the perceived "brittleness" of the hierarchical principle can be squared with evidence of growing elite assurance in the colonies remains unclear. The defining elements of evangelical localism, here described most evocatively as an "environ-

ment without an ideology" (p. 104), remain to be articulated. And the middle colonies, which recent scholarship suggests would be the acid test of the convergence and transformation thesis, receive only passing mention. Lockridge provides stimulating suggestions for a synthesis on the growth of political legitimacy in early America, but the book on the social underpinnings of political authority remains to be written.

EDWARD M. COOK, JR.
University of Chicago

EDWARD COUNTRYMAN. *A People in Revolution: The American Revolution and Political Society in New York, 1760–1790*. (Johns Hopkins Studies in Historical and Political Science, ninety-ninth series, number 2.) Baltimore: Johns Hopkins University Press. 1981. Pp. xviii, 388. \$24.50.

WILLIAM PENCAC. *War, Politics, and Revolution in Provincial Massachusetts*. Boston: Northeastern University Press. 1981. Pp. xvi, 314. \$18.95.

In rather different ways the two books under review make significant additions to our knowledge of the American Revolution in two colonies.

"War," William Pencak writes, "is the starting point for understanding royal Massachusetts" (p. xi). War is also, he argues, critical to comprehending Massachusetts's unique contribution to the American Revolution. The colony's involvement in the French and Indian wars was total; its citizens experienced conditions approximating modern "total war" (pp. xi–xii). War taught Massachusetts to mobilize people and resources. It bred political leaders skilled in wartime leadership and committed to the aims of the imperial authorities. It also laid the basis for colonial resistance to Britain after 1763. Continuous war or the threat of war produced a pro-British "court" faction, but peace generated a radical, antiwar, "popular" party. When peace brought British demands for more taxes for American defense and criticism of past colonial war efforts, popular opposition turned to outright rebellion. In Massachusetts revolution was "the reasonable response to severe, undeserved burdens" (p. xii). Because the "court" faction was held responsible for pro-British wartime policies, the revolution became radical, led by new, "obscure" (p. xiii) men unconnected with past administrations. But Massachusetts's revolution "was not radical in a Marxist sense" (p. xiii). War created unity; peace brought united resistance. In neither circumstance does the author find evidence of class conflict. The colonists, Pencak concludes, "combined against a group defined politically, not economically. A society united to oppose its government" (p. xiii).

Pencak's emphasis on war as a cause of the revolution is interesting but not wholly convincing. He needs to clarify exactly what "total war" meant in the context of eighteenth-century Massachusetts, and how "total war" affected all the colonists' lives, not just the careers of the leadership. Pencak's brief "speculative" (p. 203) excursion into psychohistory to explain generational differences between Tories and patriots (pp. 202–06) adds little to the argument. The notion that the revolutionaries harbored a secret wish to kill their fathers is rapidly becoming an overworked cliché. The role of warfare and the military in the coming of the revolution has, however, been too often neglected, and, these criticisms notwithstanding, Pencak's is a welcome book.

Unlike Pencak, Edward Countryman grounds his excellent account of revolutionary New York in a detailed analysis of class conflict and discontent. Countryman focuses on the process of revolution, the essence of which he sees as "an explosion of political participation" (p. 295). The result is one of the best examples yet to appear of the so-called new social history. The book is divided into three parts. Part 1 examines the decline of the colony's old order. Part 2 looks at the revolutionary crisis. Part 3 explores the "revolutionary redefinition of New York" (p. xiii), and it is here that Countryman makes his most valuable contribution. The redefinition took several forms. Foremost "was the forcible separation from New York of whole groups of people who stood opposed either to what the province had been or to what the state was becoming" (p. 283). These included the Tories, but also the Vermonters, whose independence "was the one occasion in early America when agrarian rebels got what they wanted" (p. 284). A number of constitutional and institutional changes were also important. These were facilitated by the state constitution of 1777, which Countryman perceptively characterizes as "middle-of-the-road" (p. 285) and which enabled New York to avoid the extremes of left and right experienced by Pennsylvania and Maryland respectively. Lastly was the compromise achieved between conservatives and radicals, the acceptance by both of the legitimacy of political partisanship. The triumph of national conservatism in the constitution of 1787 effectively checked state radicalism; conservatives meanwhile faced up to the reality of popular, egalitarian politics. Countryman observes that "it was precisely the combination of new men, radical policies, and unexceptional institutions that stabilized the new order within New York" (p. 287).

Countryman's use of quantification as applied to legislative data to trace the development of partisanship in the state assembly during its first eleven sessions (part 3 and appendix) is impressive, as is his explanation (pp. 103–30) of the divisions between revolutionaries, loyalists, and those caught in the

middle. Less persuasive is his analysis of revolutionary crowd behavior. Quoting Jesse Lemisch, Countryman argues that crowds provided the "sustained militancy" to create "fully revolutionary politics" (p. 37). The evidence, including a detailed list of notable riots or disturbances (pp. 37–45), hardly adds up to a convincing picture of specifically revolutionary behavior. To be sure, rioting in town and country occurred frequently; nor was participation confined to any one class or faction. Indeed, riots were apparently so commonplace that we may wonder if they had any significance at all. Certainly their meaning remains ambiguous. Countryman's attempt to link American crowds to a broader tradition of European crowd activity, as defined in the work of such writers as George Rudé, Eric Hobsbawm, or Edward P. Thompson, is at best only partially successful. Ironically, Countryman's study may well point to the uniqueness of the American revolutionary experience.

LEOPOLD S. LAUNITZ-SCHURER, JR.
University of Queensland

ERNA RISCH. *Supplying Washington's Army*. (Special Studies, Center of Military History.) Washington: Center of Military History, United States Army, 1981. Pp. xiv, 470. \$13.50.

The reawakening of scholarly interest in the military history of the American Revolution that has taken place over the last two decades has led to considerable attention being focused on civil-military relations, the impact of the Revolutionary War on social and economic institutions, and reassessments of army officers. As Brigadier General James L. Collins, Jr., notes in the foreword to Erna Risch's book, however, there is in all this outpouring of military history "little more than fragmentary coverage of [the army's] supply system" (p. v). Risch admirably repairs this deficiency by covering in exhaustive detail the story of how institutions designed to keep America's armies in the field were organized and operated during the Revolution. The author brings to this work extensive experience as a scholar in the historical sections of the Quartermaster General and the Army Materiel Command, and her long association with these services is reflected in her understanding of how George Washington's army was fed, clothed, armed, and transported.

The five supply services of the Continental Army—the quartermaster's department, the commissariat, the clothing department, the ordnance department, and the hospital department—are the focus of Risch's analysis. She surveys the establishment and evolution of these agencies, discussing at the same time the turnover of key personnel within them and the role of the Continental Congress in

contributing to their viability or lack thereof. A key point that Risch emphasizes throughout the book is the haphazard way in which all the departments actually functioned, despite the way they were "supposed" to, according to law and the organizational charts. She notes that there was much innovation in the service departments themselves and also in the way commanders from time to time secured supplies or transport on their own. She marks the importance of French aid to the American cause, and she objectively points out—in contradiction of a considerable amount of pious nonsense by other writers—that the Revolutionary generation was motivated by both acquisitiveness *and* patriotism in the services of supply, not solely one or the other. This state of affairs she finds neither remarkable nor (apparently) out of character with how humans act at all times in history.

Based on sound, extensive research in the papers of the Continental Congress and the Revolutionary War records in the National Archives, the Washington papers in the Library of Congress, various important private historical collections, printed primary sources, and a wide-ranging list of secondary works, Risch's book is a model of research. It will be welcomed by scholars of the Revolutionary War and other military historians as the standard reference work on how the Continental Army was maintained in the field during the war against Britain.

PAUL DAVID NELSON
Berea College

DAVID NOEL DOYLE. *Ireland, Irishmen, and Revolutionary America, 1760–1820*. Dublin: Mercier Press, for the Cultural Relations Committee of Ireland, 1981. Pp. xix, 257. £4.95.

This volume gathers together for the first time and from scattered secondary sources the comprehensive story of Ireland and the Irish as they related to the American Revolution. The book constitutes a well-executed and fitting salute by the Republic of Ireland (its sponsor) to the American Bicentennial. The thoroughness of its author, historian David N. Doyle of University College, Dublin, excuses the tardiness of its appearance.

Besides offering his own organization of the field, Doyle has brought together a twenty-six-page annotated bibliography compiled from both sides of the Atlantic. The text itself rests almost exclusively on judicious use of standard and obscure secondary materials, and from this two-hundred-year literature has come a broadly conceived work. The volume's unfortunate shortcomings are those that easily could have been remedied—no maps and no index.

The central topic is the role of the Irish (Ulster or

Scotch-Irish, Anglo-Irish, and native or Gaelic Irish) who lived in America during the Revolution. Resting as it does on standard sources, Doyle's general analysis contains no surprises. He does, however, identify and explore quite clearly the eighteenth-century divisions among the Irish and the local American environments in which they functioned, and he judges the reasons for their actions.

The criticism of the data contained in Michael O'Brien's *A Hidden Phase of American History* (1919) should serve as a proper antidote to ethnocentrism. The Irish, mostly the Ulstermen who constituted the overwhelming proportion of Irish in the colonies, did fight and die in large numbers but hardly to the exclusion of other revolutionaries.

The more substantially based record of Irish achievement in the naval service is accepted even though Captain John Barry, a symbol for many subsequent Irish-Americans, is all but ignored. Perhaps as an intellectual peace offering to present-day Ireland, Doyle's candidate for the greatest Irishman of the American Revolution had to be George Bryan, an Anglo-Irish Presbyterian and an abolitionist whose labors for human freedom overcame Ulster-Irish indifference to black slavery in Pennsylvania.

Auxiliary chapters set the stage in Ireland and North America for the coming of the Revolution and then trace the results in Ireland and on the Irish in America. Interestingly, the author (whose previous work examined nineteenth-century Irish America) asserts that the disadvantaged condition of the Irish in the colonies improved markedly following the Revolution and that the plight of the emigrants who fled the famine of the 1840s actually constituted a dismal variation in the larger pattern of Irish adjustment to American life.

Under the gaze of any perceptive researcher the Irish of one tradition or the other tend to appear just about everywhere in American history. This should be expected because the Irish were the first of the nation's immigrant groups, and, in proportion to the total populations of the sending countries, the Irish were the largest. The challenge to any historian dealing with the American Irish in the making is to determine the major theme to which such an abundance of already available material may rightly be subordinated. Here and there Doyle wandered, but that too is consistent with the Irishmen he pursued.

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JAMES LAL PENICK, JR. *The Great Western Land Pirate: John A. Murrell in Legend and History*. Columbia: University of Missouri Press. 1981. Pp. viii, 197.

James Lal Penick, Jr., suggests that "not one in five" (pp. 175-76) professional historians today would recognize the name of John A. Murrell, the subject of this intriguing book. A closer estimate may be not one in fifty or five hundred, for the legend of John A. Murrell is now all but forgotten.

His life was scarcely the stuff of which legends are made. In the 1820s and 1830s, Murrell sometimes scratched the soil of central and western Tennessee as a farmer, but he invested most of his energy in other pursuits. He and his three brothers seemed forever serving time or escaping prosecution for perjury, forgery, larceny, counterfeiting, horse stealing, gambling, or "riot." One brother died cheating at cards; Murrell closed his career with ten years at hard labor, framed for slave stealing. But within two years of his conviction this feckless ne'er-do-well was being celebrated as a "land pirate," slave-stealing abolitionist, mastermind of a vast criminal network, and organizer of a massive slave insurrection.

This transformation in his stature came about through no efforts of his own. Rather, Virgil Stewart, the sole prosecution witness, needed to make the questionable conviction stick and to divert charges of theft against himself. To this end he published a pamphlet "revealing" Murrell's fantastic scheme. In 1835 his endeavors reached a tragic conclusion in central Mississippi, which was already tense with fears of a slave insurrection. The cockamamie story of Murrell's slave conspiracy helped trigger a witch hunt in which dozens of whites and blacks were killed by mobs and committees of safety. Others were flogged, tarred and feathered, and banished. The fear that whites were behind the feared slave insurrection meant that no one was really secure. The victims of mob action tended to be newcomers to the area (especially Northerners), those who had denounced slavery, or those simply disliked by their neighbors. Transients of all sorts—whether gamblers, "steam doctors" administering herbal medicine, or merchants—were under special suspicion.

Penick's stated goal is to explain how and why Virgil Stewart inflated Murrell's reputation to such outrageous proportions and to examine the consequences. But his search for hidden documents, his skeptical reading of well-known sources, and his reconstruction of complex events have produced a thoughtful study that contributes to our understanding of many aspects of life under the slave regime. While sorting out the contradictions in the court records, personal accounts, newspaper reports, and popular legends, Penick graphically depicts the turmoil, lawlessness, and insecurity of life in the lower Mississippi Valley in the 1820s and 1830s. "Border misfits" undermined the bases of

society through counterfeiting, slave stealing, and an imaginative array of other crimes. In response, mobs and vigilance committees dispensed their own version of law and order. Where rumors and fears abound, even a common criminal may seem a masterful leader of men.

MICHAEL A. FLUSCHE
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ROBERT A. TRENNERT, JR. *Indian Traders on the Middle Border: The House of Ewing, 1827-54*. Lincoln: University of Nebraska Press. 1981. Pp. xiii, 271. \$17.95.

ROBERT L. BEE. *Crosscurrents along the Colorado: The Impact of Government Policy on the Quechan Indians*. Tucson: University of Arizona Press. 1981. Pp. xix, 184. Cloth \$18.50, paper \$7.50.

Indian Traders on the Middle Border is an important book that should be read by anyone interested in how our Indian policy really worked during the years that the House of Ewing operated, 1827-1854. The firm was involved in the activities of a number of tribes in this period, particularly the Miamis, the Potawatomis, the Santee Sioux, the Winnebagos, and the Sac and Fox. The two Ewing brothers displayed unusual business acumen and an awesome, if repellent, ability to milk every situation for the maximum profit, whatever the impact on the Indians, business competitors, or even longtime associates.

Although the firm began operation with a concentration on the fur trade, it quickly moved into other areas as the opportunity arose—furnishing merchandise to tribes enriched by the sale of lands, contracting to remove Indians to new homes in the West, purchasing land from tribal leaders who had received it as bribes to accept removal treaties, and acting as claims agents for tribes and for other traders. The Ewings were beneficiaries of a number of the treaties by which the tribes sold land to the United States. Virtually every such treaty included a provision for annuities, most of which ended up in the tills of the traders, and for settlement of traders' claims for credit already extended to members of the tribe.

The Ewings did not alter fundamental policy. If they and their competitors had not existed, government policy would still have been designed to satisfy the land hunger of the whites at the expense of the Indians. What the Ewings did was to exploit the policy in every conceivable way, with no concern for the impact on the native Americans. The Ewings facilitated the negotiation of removal treaties if they stood to profit from them; they opposed others if

that was the profitable thing to do. The House of Ewing, like the American Fur Company and other trading firms, had a powerful impact on the implementation of United States Indian policy, as Robert A. Trennert, Jr., makes abundantly clear in this meticulously researched and well-written study.

Robert L. Bee's book deals with the experience of a single tribe and a relatively minor one at that. Nevertheless, *Crosscurrents Along the Colorado* is a useful ethnohistory of the application of government policy to the Quechans. As with most tribal histories, it is a dismal account of mismanagement compounded occasionally by malfeasance by both white officials and tribal leaders. Bee is particularly good at following the intricacies of tribal politics. He also makes a contribution in his discussion of Quechan experience with the economic development programs of the 1960s and 1970s. Bee imposes on the usual litany of reservation Indian problems the thesis that the Quechan condition is the result of internal colonialism, designed intentionally or otherwise to provide a pool of cheap labor for their enterprising non-Indian neighbors. That this has been the net result is patent; what is missing from this account is a viable alternative policy that might have been followed.

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ROBERT V. REMINI. *Andrew Jackson and the Course of American Freedom, 1822-1832*. Volume 2. New York: Harper and Row. 1981. Pp. xvi, 469. \$20.00.

In the preface to volume 1 of his newest biography of Andrew Jackson, Robert V. Remini criticizes an earlier biographer for not being "particularly instructive about the era in which Jackson lived" (p. xi). Whatever his book's strengths, and it is by no means devoid of them, in the two volumes published to date Remini himself has surprisingly little to say about the era's economic, social, intellectual, religious, technological, and cultural developments. And in focusing almost entirely on politics, volume 2 largely ignores politics on the state and local levels, offering instead yet another detailed description and a reinterpretation of the great set pieces of Jacksonian national politics: the "corrupt bargain," the Peggy Eaton affair, Indian removal, nullification, the Bank War, and Jackson's use of the veto power.

In his preface to the volume under review, Remini writes that "the problems of understanding the period from 1816 to 1848 stemmed from a failure among historians to appreciate the role and contribution of the man most responsible for shap-

ing that age" (p. x). It is not clear that Remini's evaluation of Jackson's "role and contribution" will put an end to historians' "problems of understanding" the second quarter of the nineteenth century. What is more certain is that specialists will find Remini's discussion interesting, for it offers a new interpretation of Jackson's political role.

In Remini's view, "the Age of Jackson . . . if it is to make any sense at all, should be seen as the first conscious effort at political reform in American history." The previously misunderstood Andrew Jackson emerges in these pages as the great reformer, a devotee of the republican "ideology" of the founding fathers, dedicated above all to restoring "the country and its government to virtue and honesty" and protecting and perpetuating "the liberty of the American people" (pp. ix-x). It seems that the Monroe and John Quincy Adams administrations were cesspools, the one of corruption, the other of despotic menace to American freedom. Fortunately, Jacksonian champions of "public virtue" rose to the occasion, led by the towering figure who was simultaneously Old Hickory or unparaleled Indian fighter, The General or Hero of New Orleans, and, it now turns out, a deep political thinker. (As Remini pointed out in volume 1, it is possible that Jackson may have read some of the books he owned.) This is indeed a novel thesis, testimony to the ingenuity of its author. That he argues it unpersuasively, relying on partial evidence and strange readings of the evidence on Jackson's predecessors in the White House, on the one hand, and a most naive reading of the politic statements made by Jackson and his political followers, on the other, does not detract from the fact that he argues it heroically. For, as Remini makes clear he understands, much of the actual public performance of the Jacksonians in power offers no sustenance to the thesis that they were men of high principle.

As a highly informed participant in the scholarly controversy about the meaning of Jacksonian democracy, Remini is aware of the foibles, the inconsistencies, and the unattractiveness, whether of the policies or the personalities and character of the Jacksonian leaders. In view of his great admiration—if not adoration—of Jackson, Remini's painstaking recital of the man's bizarre and even lunatic behavior, dangerous egomania, penchant for violence, and what he calls his "inane," "idiotic," and "disgraceful" policies is admirable. While it betokens too the intellectual sophistication of the author, it does not make more convincing his ultimate argument that, weaknesses and inconsistencies notwithstanding, the Jacksonian movement was essentially what its leaders said it was.

At his best, Remini tries to have it both ways. The Hero's inconsistency shows he was never dogmatic. Jacksonian Indian policy may have been "harsh,

arrogant, racist," but it was also inevitable, sincere, highminded, popular. Jackson's performance in high office subverted the concept of liberty but it did so "unconsciously," in response to alleged "new impulses in American society," and "assisted in the conversion of a republic into a democracy" (pp. 323-324). At its worst, the volume lapses into partisanship, as in its almost invariably invidious descriptions of the motives and behavior of Jackson's opponents or its manner of documenting sweeping sympathetic portrayals of Jacksonians and their policies by reference to what one or more Democrats had to say about the matter.

Remini offers no bibliography, but his preface to volume 1 and his notes indicate that he has read widely in the private papers of Jackson and other contemporaries as well as in the scholarly literature on national politics. At times he relies uncritically on what Parton or such partisan contemporary sources as Eaton, Kendall, Taney, Benton, Blair, and Van Buren had to say (in some instances long after the fact) about an important issue. Although the notes offer corrections of such esoterica as the birth date of Jackson's adopted son, the date of death of his "adopted" Indian boy, and the first name of an obscure government clerk, the book appears to be aimed at the general audience. At least that is my interpretation of its failure to correct Jackson's spelling and grammar (a decision that no doubt saved the publisher much ink), its attempts at purple prose, its many references to Jackson's "blazing eyes" and to the interesting fact that at times he evidently "glowed" and "vibrated with excitement," Benton "sizzled with passion," and to the artful short sentences strewn throughout the book. Chapter 8 closes with: "He could not blot it from his mind. Andrew Jackson never forgot."

Fortunately, the book is for the most part well written. Its chief problem is that in straining to present biography as thesis, its unconvincing attempts to sustain an unpersuasive thesis detract from what is usually an informed as well as controversial discussion of the national political themes that seem to fascinate the author.

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JONATHAN D. SARNA. *Jacksonian Jew: The Two Worlds of Mordecai Noah*. New York: Holmes and Meier. 1981. Pp. xi, 233. \$29.50.

Jacksonian Jew: The Two Worlds of Mordecai Noah by Jonathan D. Sarna is a prodigiously researched biography of Noah, one of the better-known Jews of

antebellum America. Noah used his various talents to build moderately successful and influential careers in politics, diplomacy, journalism, and theater, as well as to claim leadership in the fledgling American Jewish community of his day. The highlights of his life were a brief, controversial consulship in Tunis during the Barbary pirate era; his role as a leading figure in the New York Jacksonian Democratic party; his editing of the New York *National Advocate*, the New York *Enquirer*, and the New York *Evening Star*; his authorship of such successful but rather formalistic plays as *She Would Be A Soldier*, *Marion*, and the *Grecian Captive*; and his sponsorship of a Jewish refuge named Ararat on Grand Island in the Niagara River. Despite energetic attempts, however, Noah never achieved marked success in any field. Sarna, succumbing to the biographer's nemesis—overidentification with the subject—often exaggerates the achievements, successes, and abilities of Noah, losing a sense of balance and perspective while placing him in the context of American political and journalistic history and the American Jewish context. My belief is that many individuals who were not as public as Noah achieved more than he did.

As the subtitle indicates, Sarna uses Noah's life to explore the tension between the American scene on the one hand and the American Jewish community on the other. He does this with skill and insight, and this is perhaps the greatest contribution of the book. As Sarna describes, Noah "embodied but could not resolve the tension faced by all minority groups: the simultaneous search for the benefits of wholehearted assimilation and the security of ethnic-roots identification." To Noah's credit, in his efforts to resolve this tension, he apparently never hid his Jewishness. In fact, Sarna argues that "to the end [Noah] remained a conscious, loyal, and proud member of the Jewish community." Noah's contribution lay not necessarily in the success of his efforts at assimilation, but in making, for the first time, his Jewishness a relevant factor in his private and public life. In the process, he articulated and lived the challenge that his American Jewish contemporaries and future generations of American Jews, as well, would face.

There is no doubt that Sarna's study is the best account yet published on Noah and should remain the standard work for some time to come. Sarna's research is substantial, his narrative careful and thorough, and his analysis sharp, broad, and insightful. There are, however, some problems. Its greatest weakness is its occasional celebratory tone. Notwithstanding Noah's accomplishments, I cannot agree with Sarna that Noah's achievements "raised him above his contemporaries and transformed him into an historical figure of enduring importance." What we are dealing with here is essentially a

"second-rate" politician and journalist with inflated pretensions of greatness. Sarna tells us a great deal about what Noah said, wrote, and accomplished, but he is unable to convey the essence of the man, the nature of his personality and the impulses that drove him—not an easy task for any biographer. One of the more important of Sarna's contributions was the reminder that antebellum America was not free of antisemitism. More could have been done, however, with this theme. Nevertheless, despite its faults and gaps, the well-written *Jacksonian Jew* is ultimately a significant contribution because it gives us some insight into the career of an interesting early figure in the American Jewish experience.

MICHAEL N. DOBKOWSKI

Hobart and William Smith Colleges

DAVID J. JEREMY. *Transatlantic Industrial Revolution: The Diffusion of Textile Technologies between Britain and America, 1790–1830*. Cambridge, Mass.: MIT Press. 1981. Pp. xvii, 384. \$32.50.

For several years now, David J. Jeremy has made the transfer of technology between Britain and America his special field of study. Those of us who have followed the series of articles appearing in a variety of journals since 1971 have long awaited the publication of his full-scale study, and probably few of us were surprised to learn that it had been awarded the Dexter Prize for 1981 by the Society for the History of Technology. The work is a major contribution to the history of technology and, I believe, to the broader field of American history in general. By his focus on two basic questions, Jeremy has written a book that inevitably goes beyond a narrowly construed history of technology and leads into matters of broad cultural interest. First, he has asked, what specific circumstances hindered or promoted the spread of the technologies? Second, he has inquired into the conditions in America that prompted the modification of the technologies.

The book is explicitly conceived as an effort to test, by a close study of a particular case of technology transfer (that of the cotton and woolen industries that led the way in America's industrial revolution), some of the broader generalizations and controversies regarding American technology. In a brief review, one can do little more than indicate some of his major conclusions. One finding has to do with the transfer of technical know-how by immigration, and it is not surprising to learn that Jeremy places supreme importance on the movement of artisans themselves. The key idea on the American scene was to allow the British to bear most of the expenses associated with innovation, a tactic that worked admirably in the early phase of the industrial revolution. In the mechanical technology of the late

eighteenth century, upon which Jeremy concentrates, success did not depend on technical innovation but simply on finding out about the state of the art and bringing American factories up to it. Moreover, the technology was complex enough that importing machines without men was simply inadequate. There were cases of early manufacturers having to abandon imported equipment because they lacked experienced men to build, run, repair, and manage it. By hiring experienced managers from England, even at high wages, manufacturers found that they could avoid the slow and expensive process of learning by doing—as the British had done in the 1770s and 80s when textile technology spread throughout the British Isles. But Jeremy's broadest contribution in discussing immigration is probably the careful distinction he makes between individual immigration and aggregate immigration. The typical British immigrant of this period brought obsolete skills and contributed little to the industrial revolution. The technology transfer was accomplished by a handful of men—perhaps no more than a half-dozen—who did not conform at all to the typical pattern; they tended to be middle-aged men with managerial or mechanical skills.

Although the technology of America's industrial revolution was brought here by foreign artisans, it is well known that the imported technology was so reshaped to fit local conditions that a distinctive "American system" involving standardized production lines, greater mechanization, and interchangeability had evolved before the 1850s. While generally agreeing with H. J. Habakkuk that labor scarcity due to an abundance of land produced a pattern of labor-saving innovation, the author also shows how other factor influences, market conditions, and the needs of the imported technology itself played important roles, and he stresses the fact that somewhat differing patterns of innovation characterized the different manufacturing centers. The best example of reshaping a technology to fit local conditions is found in his discussion of woolen manufacture, in which all the elements conspired to produce an emphasis on quantity production rather than quality, and in which subsequent innovations reinforced and extended this emphasis.

Jeremy has confined himself in this study to four technologies covering the mechanical manufacture of cotton and woolen cloth: cotton spinning, cotton power-loom weaving, calico printing, and woolen manufacture. He excludes such considerations as chemical technology, technologies specific to other fibers, the provision of power sources and transmission systems, and the whole question of the role of the machine tool industry. Perhaps there are those who will regret especially the final two omissions, for had he considered such details his conclusions might have been different, but I really have to endorse the

author's decision to restrict his focus. By doing so, and by asking major questions of his limited material, Jeremy has made the best contribution to date to our understanding of technology diffusion in a developing area. To note that virtually every page of the book suggests some further unsolved problem is only to highlight the importance of the subject and the value of this work.

GEORGE H. DANIELS

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BROOKE HINDLE, editor. *Material Culture of the Wooden Age*. Tarrytown, N.Y.: Sleepy Hollow Press. 1981. Pp. 394. \$22.50.

For more than two hundred years wood was the dominant natural resource in American material culture, and the vast array of wooden products cut, burned, designed, shaped, and manufactured by Americans were not mere signs of economic well-being but reflections of spiritual ideals, rules for social behavior, and attitudes toward the environment. The ten essays in this volume—a companion to *America's Wooden Age*, edited by Brooke Hindle, and published by the same press in 1975—cover most of the important forest products prevalent in our culture before the age of metals and synthetics: farm equipment, building frames, ships, wooden roads, bridges, railroad equipment, fuel, potash, industrial machinery, and charcoal.

Every essay contributes some new knowledge of processes that disappeared during the last half of the nineteenth century and the early decades of the twentieth century. There is a consensus, moreover, on the gradualness of technological change, and the essays support the contention of recent historians of technology that transitions in techniques and the use of materials tend to be relatively slow. Older technologies often are improved even after new technologies are widely accepted. Richard Shallenberg, in one of the more important essays, presents us with the example of the charcoal iron industry, which increased production until the 1890s—more than thirty years after the introduction of mineral fuels and the Bessemer converter. Even after the charcoal iron industry lost ground to open-hearth technology, charcoal production continued to grow as a by-product of the wood-distillation industry.

Two other essays deserve special mention: Dell Upton's work on traditional timber framing, and Gary Kulik's analysis of the use of wood in the first American textile mills. Americans used more wood than Europeans, but labor scarcity led to techniques aimed at minimizing its preparation. Both authors suggest that the use of vertical plank siding on buildings—boards often cut by sawmills in the Northeast—reflects the labor problem. According to

Upton, the early American attitude toward labor-conservative simplicity was the most important factor in the development of balloon frame technology in the nineteenth century.

Both Upton and Kulik are adept at distinguishing between form and function in building technology. Although the function of American structures was determined by basic needs and economic necessity, the form or design of buildings, and the materials used, were strongly influenced by spiritual and ethnic forces, and by social attitudes toward conspicuous consumption. Upton, for example, shows that some elements of Dutch building design continued on in New York in the industrial era. Kulik suggests that early textile manufacturers in Rhode Island designed simple, wooden factories to allay animosity toward the new industrial order and defuse social tensions.

CHARLES F. CARROLL
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SUSAN E. HIRSCH. *Roots of the American Working Class: The Industrialization of Crafts in Newark, 1800–1860*. Philadelphia: University of Pennsylvania Press. 1978. Pp. xx, 170. \$14.00.

The importance of the early nineteenth century to labor history and to the development of the American labor movement is now receiving needed attention. Susan E. Hirsch's slender, tightly written book, through a description of the industrializing process in Newark and its effects on artisans touched by it, makes a significant case for the centrality of the antebellum years.

Perhaps the most impressive aspect of this book is its depiction of the transformation of eight different crafts—carpentry, blacksmithing, shoemaking, saddle making, jewelry making, trunk making, leather making, and hatting—as Newark, once a quiet Puritan village, emerged as a major manufacturing center. Hirsch points out the unevenness of change: some trades such as carpentry and blacksmithing remained generally traditional with artisans yet shaping entire products with their own hand tools. In so doing they exerted some control over their wages and retained reasonable expectations of financial independence. Other crafts such as shoemaking attained a first stage of industrialization in which the various tasks were divided among workers. A second stage, reached by leather and saddle making included the use of simple, human-powered machines, while the third, reached largely by hatting and trunk making, required the use of power machinery within factories. Workers in advanced trades no longer owned their tools or product, nor controlled their remuneration or had much chance of ever owning their own shop. While Hirsch does

not totally prove that wages were reduced, it is clear the journeymen's positions were much weakened and that they became strongly differentiated from their employers.

Industrialization in Newark did not see women and children employed in factories. Rather, as cheap and less-skilled labor was needed, immigrants picked up the slack, particularly in the most industrialized trades. Moreover, as the household ceased being a source of production, journeymen saw themselves as family breadwinners and were undesirous of having their wives working. Too, working men married earlier, had fewer children, and (as apprenticeship declined) had their children at home longer. That the journeymen were emulating Victorian lifestyles of the bourgeoisie is not proven, but the speculation herein deserves consideration.

The last chapters of the book reason that workers, while aware of their deteriorating marketplace situation, were inhibited from taking decisive action by a deficient ideology that placed their interests as producers as one with their employers and opposed to accumulators (bankers and merchants). The reality was that manufacturing employers were part of a new capitalist alignment that severed common interests. Too, the presence of ethnic and cultural differences among tradesmen and corresponding residential segregation, particularly between Protestants and Roman Catholics and between Irish immigrants and native craftsmen further stifled concerted action. Still, working men did form trade associations that were effective in times of prosperity but quickly withered in depression. A limited ideology and lack of unity prevented any lasting political or legislative clout.

This book is most valuable in its interpretation of the 1850 and 1860 manuscript censuses, revealing the process of industrialization and changing family and residential patterns of working men and immigrants. Less original but still valuable are descriptions of trade union and political activities, which in general agree with the conclusions of Bruce Laurie for Philadelphia, though the reliance on the census and local newspapers restricts a greater analysis of religion, evangelicalism, and cultural attitudes. Too, there is little awareness of the considerable industrial development prior to 1830 in nearby New York City. Still, this is a stimulating and valuable book including a vibrant and provocative conclusion, pointing to the critical importance of this era.

HOWARD B. ROCK
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CHARLES M. COOK. *The American Codification Movement: A Study of Antebellum Legal Reform*. (Contributions in Legal Studies, number 14.) Westport, Conn.: Greenwood Press. 1981. Pp. xi, 234.

The American Codification Movement tells the story of the movement in the early nineteenth century to reform American law by reducing it to a code. Those who dreamed of a thorough, root-and-branch reform did not, of course, achieve their goal. But the movement did have some partial success, especially with regard to civil procedure. New York adopted David Dudley Field's code on that subject; but it rejected his other codes. Many of these found a home in the far west, but that chapter of legal history lies outside of Charles M. Cook's period and interests.

The main outlines of the story are well known, but Cook brings some fresh information and what were to me at least some new slants on the subject. In particular, he draws on some half-forgotten sources to show the intellectual roots of the movement. He traces the influence of French codification on American reformers; he examines the extent to which Bentham and his ideas had an impact. Cook stresses the work of jurists who wrote and argued before Field entered the lists. This leads him to a revision, or reassessment, of Field's place in legal history. Field, for Cook, was important because he was a "relentless" promoter of codification; but he basically "added nothing new" to the cause, intellectually speaking. By the time Field "came to participate in the movement," the effort had actually "begun to ebb." No "broad-based movement favoring general codification existed after the mid-1840s." There was general agreement that "partial codification" was "acceptable as a method to improve the legal system, but general codification was not" (p. 188).

All well and good. Still, Field did make a mark, even without a "broad-based movement"; and his predecessors left so little behind that it remained for Cook to write a book and rediscover them. It is doubtful anyway, whether there ever was a "broad-based movement." In the first place, are the legal manifestos, newspaper editorials, and pamphlets that Cook has dug up really enough to suggest anything so grand as a "movement"? Secondly, as the book itself makes clear, codification enthusiasts were a varied lot. Most of them, moreover, were themselves lawyers, and they were "strongly attached to the common law" (p. 83); they wanted efficiency, system, reform, not revolution. The more "radical" reformers wanted to get rid of the lawyers, as much as possible. They wanted to return to "unadorned first principles of the law" (p. 84). Yoking these various groups together does not produce much of a "movement" either.

This is a useful book, and it tells us things we did not know before. But we must remember its limits. There is really no discussion of the economic or social background of reform. This leaves us floating in the rather thin air of lawyers' squabbles. But it is

precisely in the outside world that we might find clues as to why reform failed—and why it later (in part) succeeded. Was all this furious scribbling only an abstract, and rather dreary, debate about legal philosophy? Were there no real oxen to be gored? A fuller account of the "movement" might provide some clues that this book, regrettably, leaves out.

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TONY FREYER. *Harmony and Dissonance: The Swift and Erie Cases in American Federalism*. (New York University School of Law Series in Legal History, number 4; Linden Studies in American Legal History, number 2.) New York: New York University Press; distributed by Columbia University Press, New York. 1981. Pp. xv, 190. \$22.50.

One of the most complex aspects of American federalism relates to the diversity of citizenship jurisdiction vested in the national courts by Article III of the Constitution. Since most diversity cases involve questions of local (state) law, we have been concerned throughout American legal history with the question of whether federal judges are obliged to follow state common law in commercial cases or are free to make their own rules of law in such matters. In Section 34 of the Judiciary Act of 1789, the "Rules of Decision" statute. Congress directed the federal courts in diversity cases to regard "the laws of the several states" as supplying rules of decision, "except where the Constitution treaties or statutes of the United States shall otherwise require or provide." A considerable history of how the Supreme Court came to grips with this problem is reviewed by Tony Freyer. This history reached its climax in 1842 in *Swift v. Tyson*, 16 Pet. 1, in which Justice Story announced for the court that the phrase "the laws of the several states" refers primarily to statute laws and that where there is no specific state statute on the subject, or "local usages of a fixed or permanent operation," then federal judges in diversity cases are free to do what state judges can do, namely, pronounce rules of common law. Clearly, as Freyer points out, Justice Story was eager to promote a body of uniform national commercial law as the commerce of the nation became increasingly national in character.

Ever since its inception, the propriety of the *Tyson* decision has been a subject of serious debate among legal scholars, lawyers, and judges. The main objection was that the *Tyson* doctrine permitted different rules of law to prevail in the same area, depending upon whether a state or federal court had the case. This also promoted what is known as "forum shopping," an advantage largely available to corporations, which have always had freer access to the

diversity jurisdiction of the federal courts. The debate over this rule reached its modern climax in 1938 in *Erie R.R. Co. v. Tompkins*, 304 U.S. 64, when Justice Brandeis, speaking for a divided court, ruled that in following the *Tyson* rule, the federal courts had unconstitutionally invaded the reserved powers of the states. That is to say, the holding was not merely that the courts had misconstrued the "Rules of Decision" statute but that, as construed, an unconstitutional course of action had been followed.

Freyer has reviewed the history of this problem with great skill and serious scholarship. It is regrettable, however, that the author concluded his examination rather abruptly with an exposition of the *Erie* decision. He would have been well advised to review some of the leading problems that *Erie* has run into. While under the *Tyson* rule there was always the problem of drawing the line between general and local law, so also under *Erie*, there is another body of uncertainties in drawing the line between substantive and procedural law, for so far as procedural law is concerned federal courts have never been obliged to follow state law. In addition, it is regrettable that the author did not look very far into the whole question of the advisability of the diversity jurisdiction. There are hints in this book on such matters, but they are only hints. Even so, this is a well-written and well-researched analysis of an important chapter in American legal history. Thus, it is unfortunate that publication was preceded by sloppy proofreading.

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LARZER ZIFF. *Literary Democracy: The Declaration of Cultural Independence in America*. New York: Viking Press. 1981. Pp. xxv, 333. \$20.00.

Larzer Ziff has turned once again to the formidable challenge of relating American literature to the American life that nourished it. The author of *The American 1890s* and well-known studies of Puritanism, he writes here of the "life and times" of Poe, Emerson, Thoreau, Hawthorne, Whitman, and Melville. By looking closely at the thought and art of each of these writers, Ziff attempts to set their master works within the context of contemporary history and their creators' social and political convictions.

Although dwelling little on either, *Literary Democracy* focuses its attention between the Panic of 1837 and the outbreak of the Civil War—roughly the decades that F. O. Matthiessen christened the American Renaissance. Matthiessen looms large in these pages, not simply in the direct discussions of the preface and afterword, but implicitly through-

out. His initiating work provides one of the defining borders for *Literary Democracy*; the other is the graceful social portraiture of Van Wyck Brooks. Ziff sets himself "a different errand"—to explore the terrain between Brooks's evocative anecdotage and what he describes as the "purely literary," primarily aesthetic emphasis of Matthiessen. But the concerns of *Literary Democracy* and Matthiessen's *American Renaissance* cannot be distinguished so sharply. Matthiessen's close analysis of literary texts was dominated by his quest for the idea of America in its art, and he never doubted that the writing he admired drew its strength from the life lived by ordinary Americans. The overlapping assumptions of the two books become quite obvious when Ziff presents *Moby Dick* as basically a celebration of its ordinary crewman, much as Matthiessen had done forty years earlier.

But no book need apologize for echoing its most luminous predecessor; *Literary Democracy*, in fact, often surpasses *American Renaissance* in the subtlety and depth of its critical readings. The examination of Hawthorne's political views and their bearing on his conception of romance, for example, is excellent, and Ziff's skill with the less than great provides his book with notable successes in chapters on Harriet Beecher Stowe, Margaret Fuller, George Washington Harris, and George Lippard. Moreover, Ziff knows four decades of commentary on his major authors that Matthiessen never saw, and he draws as well on the fruitful debates that have come to characterize the fields of popular culture, literature and history, and American studies.

This is not to say that Ziff has solved the familiar methodological difficulties that have plagued these efforts to merge disciplines. In place of the numbing dichotomy of art and society, he posits art as a social form, causally implicated in its world of origin. This emphasis leads him to argue that the major writing of the nineteenth century was shaped more by the history of America than by the history of literature. The impulse has been a popular one: to define a unique American art as a direct—even necessary—consequence of a unique America. But that America appears in *Literary Democracy* only as a generalized setting for sharply perceived fictions, fictions that indisputably confront the same questions that have challenged writers of vastly different times and places. Those who doubt the formative centrality of literary nationality will not be convinced by anything they find in *Literary Democracy*.

But these are notoriously difficult questions. Ziff can only be praised for confronting yet again the undeniable yet persistently elusive relation between the work of art and the personal and cultural history that certainly shapes it. Less admirable is his apparent need to insist that several discrete essays make a thematic whole. Unlike Matthiessen, who

had the passion of his personal commitments to cement large portions of his huge book, Ziff has only his loosely defined "twin focuses," literature and society, to unify his much less detailed discussions. He holds to them as tightly as he can, but in the end he admits not only that his book cannot claim a center but also that his truths must perforce share the asymmetry of life—that is, I take it, that he has been defeated in his effort to "invent . . . finished . . . ideas . . . to account for" them. It is a disappointing confession with which to close what promised to be the major account of the American Renaissance for our generation, and a surprising one from such an astute judge of the formal constructions of others. But the book has other successes to boast. It may not be a conclusive discussion of its declared topic, but it offers page after page of well-seasoned commentary by one of the leading students of our national letters.

RICHARD RULAND
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DAVID S. REYNOLDS. *Faith in Fiction: The Emergence of Religious Literature in America*. Cambridge: Harvard University Press. 1981. Pp. 269. \$22.50.

David S. Reynolds has written a meticulously researched guide to the development of nineteenth-century American religious fiction, as an extension of faith and a substitute for it. The story he tells—the secularization of Victorian literary culture—is not new, but Reynolds offers an exhaustive knowledge of books and writers, both famous and obscure, relevant to his study that lends his effort the status and importance of an archaeological find. Reynolds traces the growth of an increasingly liberal, nondoctrinal, this-worldly fiction, symptomatic of a weakening Calvinist creed and largely critical of it. The less orthodox factions of the American Protestant establishment naturally led the way into fiction. By the 1820s and 1830s, Unitarians like Henry Ware, Jr., William Ware, Catherine Maria Sedgewick, and others, were turning to literature, in Reynolds's words, to "try on alternatives without arguing seriously about any of them" (p. 37), as a means of subtly downgrading theological dogma and controversy without openly taking on their opponents. Yet, by the 1850s, even their rivals in the stricter sects published religious fiction, despite their early vociferous disapproval of it. Reynolds steers the reader ably through the increasingly secular stages of the religious genres; from visionary tales, to pragmatic moralistic stories and reform literature directed against intemperance, slavery, Roman Catholicism, and Calvinism itself, and finally to the sensuous dramatizations of the Biblical era and of the Bible, polemical social gospel narratives, and chatty accounts of a comfortable afterlife.

Reynolds has a point of view that surfaces indirectly in chance definitions: he speaks of "dull sermons" (p. 14), of the "gloomy legalism" of the Edwardian school (p. 171). It is not clear whether Reynolds simply accepts liberalism as a *fait accompli* or champions it. He provides no clear statement of his opinion, no wider, potentially more revealing cultural context for his study. What were the theological debates, one wonders, that this literature was designed to criticize and replace? Yet this lack of context is perhaps appropriate and inevitable in a book intended as a monograph, a hand guide to religious literature indispensable to students of Victorian America. What one misses is rather a sense of the reality of the drama, the occasional passion elicited even strictly within the limits of the literary secularization process Reynolds discusses.

Reynolds mentions the widespread controversy among the orthodox about the very idea of religious fiction, as neither fish nor fowl, as inevitably vitiating doctrine without achieving the status of literature. Reynolds acknowledges that the major writers of the day distrusted popular religious fare, even, as in the case of Hawthorne and Twain, parodied it, but he makes little of this cultural debate or of the profound struggles with theological thinking and feeling that mark the work of Hawthorne, Melville, and Twain. Again, Reynolds merely records, without seeming to sound, the anguish of the many Victorians who could neither believe nor accept their unbelief.

Looking at the Unitarians, Reynolds stresses their conviction that Calvinism, not Arminianism, made men passive and despairing, that only a more liberal creed, one granting human activity, range, and importance, could produce a literature and a lifestyle of "heroic" moral activism. Reynolds is here providing a needed corrective for the bias of other scholars (the present author included) toward Calvinism as the "heroic" religious school; yet should one ignore the fears of impotence felt and articulated by the members of a small educated religious minority in a burgeoning semiliterate nation increasingly affiliated with revivalism sects? Why were so many of the heroic figures of anti-Calvinist literature, as Michael D. Bell pointed out in *Hawthorne and the Historical Romance of New England* (1971), women rather than men?

Many of the authors Reynolds covers, particularly the later ones, are frank, as he shows, about their aims—cash, not religious zeal. Is not this materialistic outcome of motive and theme precisely what the major authors from Hawthorne to Twain, the clerical critics of religious fiction, including those who, like Orestes Brownson, turned to writing religious novels, even the Unitarians who started the venture, feared? The Victorians were caught in a damned-if-you-do, damned-if-you-don't situation. In Reynolds's own words, they "were looking guiltily at the

theological past and furtively to the secular future" (p. 200). They were unsure, sometimes creative, and eventually exploitative in their responses to the changing religious needs of their age.

Despite the rather flat-footed quality of his book, Reynolds has many perceptive points to make, usually as undeveloped asides, insights that a thoughtful reader will wish to ponder. It would be fascinating to speculate further on the American near-obsession with angels and angelism, which Reynolds documents in prolific detail, which contemporary critics of American poetry have also noted, and which reappears today in popular American revivalism. His observation that Brockden Brown was the first novelist to make religion a psychological rather than a doctrinal literary matter, and thus stands at the head of a tradition of writers that includes Poe, Hawthorne, and James, is astute and suggestive. Finally, Reynolds notes the paradox that, as religious literature became more ecumenical, sects were proliferating—a split indicative of some indecision or partial cultural dialectic that would well repay investigation. *Faith in Fiction* is a critical study both dependable and provocative.

ANN DOUGLAS
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FRANCIS R. KOWSKY. *The Architecture of Frederick Clarke Withers and the Progress of the Gothic Revival in America after 1850*. Middletown, Conn.: Wesleyan University Press; distributed by Columbia University Press, New York. 1980. Pp. xvi, 225. \$25.00.

ROBERT MUCCIGROSSO. *American Gothic: The Mind and Art of Ralph Adams Cram*. Washington: University Press of America. 1980. Pp. viii, 294. \$10.75.

Taken together, these books on two American medieval revivalist architects cover different phases of a stylistic movement spanning the period 1850 to 1940. Both are studies in social and aesthetic conservatism. Their approaches, however, are not congruent. As an architectural historian, Francis R. Kowsky focuses on the buildings of a hitherto largely forgotten Victorian architect. As a social historian, Robert Muccigrosso directs most of his attention to the extensive polemical and historical writing that accompanied Cram's vastly more conspicuous architectural career.

Frederick Clarke Withers was an English émigré brought to the United States by the architectural publicist Andrew Jackson Downing. Addicted to the life of the English country estate despite the democratic tenor of much of his writing, Downing had already inveigled Calvert Vaux to emigrate before Withers joined the Downing establishment on his Hudson River miniestate at Newburgh. Too little is known of the important role of this rural profes-

sional office for the history of architecture, horticulture, and taste in the 1840s and early 1850s, with Downing as publicist and horticultural specialist, while Vaux and Withers (together with other architects) furnished house designs. The social historian will probably be most interested in the contributions that Kowsky makes to this episode in American culture simply by illuminating Withers's place in it.

Although Withers was generally sober in his domestic design, he was in the vanguard of those architects who turned to polychromatic architecture in America in the mid-sixties, as evidenced in his three most important secular commissions: much of the campus of Gallaudet College for the Deaf in Washington, D.C., the Mattewan Asylum in Poughkeepsie, N.Y., and—a major Victorian building—the Jefferson Street police department and jail (with firewatcher's tower) in New York's Greenwich Village. All are fortunately preserved. The bulk of Withers's work, however, throughout his career centered, as did Cram's, in church building. He published a handsome sampling of them as *Church Architecture* in 1871. When published, his tenacious conservatism made his designs outmoded, although the integrity of his work redeemed them from being passé.

Kowsky's account of Withers's career is solid and sympathetic, appreciative of Withers's considerable virtues, aware of his principal contributions, without false inflation of his achievement. One could wish that Kowsky had considered more explicitly and more theoretically the role of the self-contained conservative in American culture by contrasting his work with that of more up-to-date medieval revivalists during the last decades of the nineteenth century, by whom even Withers was eventually influenced in minor respects.

Withers was just finishing his career when Ralph Adams Cram built his first church of consequence in the early 1890s, thereby inaugurating the most successful and conspicuous career in ecclesiastic architecture in the United States (more successful even than Richard Upjohn in this specialty, more conspicuous than that of several Roman Catholic firms like Maginnis and Walsh who may have been as successful). A polemical conservative whereas Withers was passive, Cram castigated American architecture of the period 1830–80, during most of which Withers was active, as being "worse than [that] at any time or in any place recorded in history . . ." (p. 54)—a widespread attitude of the time accounting for the oblivion to which Withers's career was long consigned.

By the time Cram entered into his career, medieval revivalism had a different coloration from that which had inspired Withers, to which Muccigrosso only alludes. He relegates the architectural core of Cram's career to a single chapter and concentrates on Cram's extensive and often virulent writing on

the social, economic, and cultural deficiencies of contemporary society. These could be corrected, Cram believed, only by values inherent in medieval society, which included a revived medieval architecture adapted to modern use. But an account that disposes of Cram's central activity in passing must substantially forego analysis of the tension that existed between the social idealism of the writing vis-à-vis the actual accomplishment of the architecture. Muccigrosso does venture, but too cursorily, into the disparate uses of the Middle Ages in social theory at the end of the nineteenth century: ranging from the scorn with which Cram's fellow architect Louis Sullivan opposed a Comtean-tinged "Feudalism" to "Democracy," through the aesthetic medievalism of Henry Adams, to the affirmative views of the medieval past by William Morris and Cram himself—with the radicalism of the one containing as many conservative aspects as the conservatism of the other contained radical elements.

Although leaving some of the broader questions with respect to Cram's career underexplored, Muccigrosso provides as convenient a summary of the stream of Cram's writings as presently exists, with judicious commentary throughout. If Kowsky's study is more complete, Muccigrosso contends with the far bigger and more complex career. Both illuminate aspects of different kinds of conservatism in American culture.

WILLIAM H. JORDY
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BETTY L. MITCHELL. *Edmund Ruffin: A Biography*. Bloomington: Indiana University Press. 1981. Pp. x, 306. \$22.50.

Betty L. Mitchell proposes to give a "fresh look" at the tormented and meandering life of Edmund Ruffin, the antebellum Virginia planter known for his contributions to agricultural improvements and his virulent secessionist sentiment. The purpose of this biographical narrative is to show how Ruffin's life reflected the "temperament" of the Old South's planter aristocrats. To explain Ruffin's reformist and radical behavior the author offers the image of a sulky, sickly, and withdrawn child, who grew up feeling both neglected and persecuted. Often brilliant in its vivid description of a man's descent into madness, the work also offers an exhaustive synthesis of Ruffin's views and activities as found in his diary, letters, and many published articles.

Yet the book fails in its attempt to allow the subject to explain his own views and political activities. Although Ruffin reveals that his political and economic principles were based on the theories of John Taylor of Caroline and Adam Smith, the nature of their influence and the transformation of

ideas into actions remain undeveloped. Why Ruffin sought political office, edited magazines, or wandered aimlessly into history (he witnessed John Brown's hanging, addressed the South Carolina secessionists in 1860, fired the first shot at Fort Sumter, and "fought" at First Manassas) remains unanalyzed.

The reader also is given a poor view of Ruffin's relationships with other radicals. The complex political activities of such personalities as South Carolina's James Henry Hammond and Ruffin's Virginia colleagues are often wrongly conceived. For example, Hammond's change from radical secessionist to unionist, which the author ignores, could, by comparison, have heightened the reader's awareness of Ruffin's radicalism. Equally mysterious is why such intelligent and articulate South Carolina leaders humored and encouraged old Ruffin. In short, because Ruffin's diary is the main source for the lives of these important political figures, it is difficult to grasp either their relationship with him or their own views on secession.

The author does make excellent analysis of Ruffin's agricultural reform activities. Descriptions of the old patriarch at work in his fields and among his slaves convey Ruffin's keen business judgments. But there is no attempt to explain why so many planters praised Ruffin's agricultural theories yet ignored his advice. Again, we are left with Ruffin's own views on planter business activities.

The author neither adds new source materials nor offers a new thesis on political and economic activities to justify redoing Avery O. Craven's fifty-year-old definitive biography of Ruffin. Since little use has been made of the veritable historiographical explosion from the early 1960s on such topics as the economy, race relations, planter class awareness, and secessionist thought, the conclusion that Ruffin's thought and activities represented planter elite "temperament" remains largely unsubstantiated. Good writing and a sometimes exciting story line do not justify retelling such a well-known life.

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WILLIAM S. MCFEELY. *Grant: A Biography*. New York: W. W. Norton. 1981. Pp. xiii, 592. \$19.95.

The familiar Grant was a simple, unassuming man, more sparing of words than of his soldiers—placid, persistent, and pragmatic—all suggestive of the "strong, silent man." The Grant that emerges in this fascinating biography is an "ordinary" man with no special qualities and rather limited talents. A failure before 1861, the war released fresh energy in the future commander of all Union armies and gave

Grant a comfortable context in his own life in which he could relate to his countrymen.

William S. McFeely's *Grant* is also a man of calculation: headquarters were "studiously simple," certain letters were intended to be made public, the casual garb and excessive modesty were designed to impress others, and his ambition to become president after the war rivaled his better-known determination "to fight it out on this line, if it takes all summer." Even his trip around the world after leaving the White House was a strategy for winning a third term.

Grant is really two books. The first ends at Appomattox and centers on Grant the man. Here the author resembles Lytton Strachey in the way he blends fact and insight, in his creative use of memoirs, in structure and style. Like Strachey, he also writes from a point of view and makes his subject come alive. The reader feels that he can understand Grant, his relations with other men, with his family, and most especially with his wife. It is not clear if Grant was an alcoholic or merely a periodic heavy drinker. The account of Grant assuming command at Chattanooga is a brilliant piece of writing, but Civil War enthusiasts will probably not be satisfied with the impressionistic treatment of the campaigns and may bristle at casual references to Sherman's "ill-generated army" (p. 148) and Grant's "inept military strategy in the Wilderness" (p. 165). This book is not intended as a military biography.

The *other* book concerns Grant after 1865 and is historical in approach and content rather than biographical. Here the author stresses the issues and problems of Grant's presidency, and the man himself can often be seen only dimly in the shadows. In these chapters the author cites letters from Adelbert Ames and Amos T. Ackerman more frequently than Grant's own correspondence. He gives Grant good marks for his humane policy toward the Indians—"an honorable milestone on a road strewn with markers of national disgrace" (p. 311)—and the settlement over the *Alabama* claims. Although acknowledging Grant's efforts to protect the former slaves, McFeely wishes that Grant had done more and blames him for not using his personal influence to stamp out racism at West Point, an institution "impregnable to thought of any kind" (p. 376). Frustrated by his failure to annex the Dominican Republic and victimized by friends, family, and colleagues, Grant never enjoyed the success in peace that he had found in war. He failed even as a business man, which he considered his natural calling. In the final chapter, where Grant struggles with his memoirs in a grim race with cancer, the emphasis is again on his human qualities.

This is a fitting conclusion to a good book. The research is impressive and the scholarship sound, although some of the gratuitous asides—"one of the

reasons for war is to have an excuse to do some drinking" (p. 133), "Massachusetts people like to get other people" to admit their guilt (p. 335)—and the injection of attitudes more characteristic of the early 1970s do little to enhance our understanding of Grant or to help us accept his milieu on its own terms.

JAY LUVAS
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PHILLIP SHAW PALUDAN. *Victims: A True Story of the Civil War*. Knoxville: University of Tennessee Press. 1981. Pp. xvi, 144. \$11.95.

This book narrates an incident of guerrilla warfare in the southern Appalachians. In January 1863, Confederate troops seeking to retaliate for an attack by Unionist irregulars raided Shelton Laurel, an isolated mountain community in western North Carolina. While the raiding soldiers had been enlisted nearby, their town-dwelling officers are shown by Phillip Shaw Paludan to have differed strikingly, in prosperity, education, and ties to the outside world, from the rural mountaineers who were their target. After fighting with some of the males and mistreating women and children, the Confederates captured a number of suspected guerrillas. At the command of their colonel, James A. Keith, they shot without trial thirteen male prisoners. Despite the protest of North Carolina Governor Zebulon Vance, no one was convicted for this Shelton Laurel Massacre. Paludan's account of the episode is solidly based on historical sources and also incorporates many concepts from the social sciences, including sociology and psychology. While the latter material sometimes offers stimulating insights, it also gives the narrative a speculative quality, manifested in an unsettlingly frequent use of such words as "perhaps."

Paludan attempts with only partial success to place the Shelton Laurel Massacre within broader contexts of world and Civil War history. Interested in what he sees as the persistent capacity of humans to commit atrocities, he refers to massacres in other wars. He does not, however, demonstrate that the similarities were greater than the differences between Shelton Laurel and other mass killings. He is also less convincing when relating the Shelton Laurel Massacre to higher levels of Confederate command. He begins by attributing a heavy responsibility to General Henry Heth who authorized the Keith expedition to Shelton Laurel. Yet, after an overly detailed presentation of Heth's background, he charges the general mainly with failing to explain clearly to Keith the intricacies of military law concerning the treatment of guerrillas. Paludan suggests that Heth committed "the sin of remaining

silent" (p. 88). The evidence is weak for charging Heth with more than being unintelligent and ineffective. Also dubious is Paludan's charge of a postmassacre "cover up" (p. 104) reaching as high as the Confederate secretary of war, whom he holds ultimately blamable for permitting Colonel Keith to resign and escape trial until after the war when conviction became impossible. The evidence given is inconclusive that the Confederate military engaged in worse than bureaucratic bungling.

Questions about the book's attempts at wider significance should not cloud its contribution as a study of a representative event in a particular setting. The description of mountain society is generally good. Especially excellent are the analyses of the mountain people's very personal concepts of religion and justice. With respect to the importance of private vengeance, the author unforgettably quotes an old woman as telling her sons, "If you've got to die, die like a damned dog with your teeth in a throat" (p. 22). Through similarly colorful writing, Paludan carries out his expressed intent to involve his readers emotionally in the bloody tragedy that tensions within this society helped to produce. Certainly *Victims* is the definitive history of the Shelton Laurel Massacre, but more important it is a pathbreaking study of a principal theater of the guerrilla aspect of the Civil War. Paludan has succeeded admirably in rooting a historically neglected topic in the lives of ordinary people.

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PHILIP S. FONER. *British Labor and the American Civil War*. New York: Holmes and Meier. 1981. Pp. 135. \$24.50.

This is a potentially intriguing but frustrating and unsuccessful book. This is not because the subject is unworthy of study or because Philip S. Foner might not have something of value to say on the matter. The question of why and how much English textile workers influenced the Palmerston government not to intervene on the side of the Confederacy in the U.S. Civil War, despite the unemployment and suffering that the Northern blockade of Southern cotton exports caused in Lancashire and in the north of England, is an important one for both labor and diplomatic history. It is also one about which there has been much historiographical debate. In his early chapters Foner summarizes well much previous work on the issue. Opinions range from the view of Richard B. Morris and other historians that Lancashire weavers helped materially to restrain the natural sympathies of English aristocrats and manufacturers for the cause of the South, to the contrary—and most recently published—position of Mary Ellison, which is that British work-

ers supported the Confederacy and not the Union during the Civil War. Foner puts Royden Harrison's influential writings on the subject somewhere in between these two camps.

But the author's own contribution barely advances the debate, if it does so at all. This is partly because the book, despite its brevity, shows many signs of having been hastily and carelessly written. Much already familiar material is used. Footnotes are sometimes left out (as on p. 6); errors are left in (as on page 41, where we are treated to the astonishing statement, in connection with President Lincoln's Emancipation Proclamation of January 1, 1863, that he had made "*with your Queen* a treaty which your Senate has ratified"); and unexplained statements are made. For example, the general thrust of the work is to reaffirm the argument that the bulk of the British working classes supported the North. But we are told abruptly on page 55 that the Scottish working-class press "supported the South," without one word of explanation. This is symptomatic of the methodology of the book, which bases assertions concerning English labor's support for the Union—partly because of antipathy towards chattel slavery and partly for other reasons—almost entirely on the working-class press. Thus chapters 4 and 6, purporting to represent the voice of the London and Lancashire working classes respectively, consist of extended quotations from labor newspapers and almost nothing else. There is no subtlety, no in-depth social analysis, no serious attempt to show the reader either regional or institutional variations between different levels of English working-class opinion. The point is not that many Lancashire textile operatives did not make considerable sacrifices in lending their support to the Northern cause in the name of liberty and democracy as well as in the hope of a future extension of the franchise on their own behalf. The point is that, even more than in most of Foner's other works, we are presented with a unidimensional, "noble proletarian" form of labor history that seriously overestimates the political influence of the tiny Marxist bodies operating on both sides of the Atlantic at this time.

Perhaps the most glaring example comes at the end of the book, where the author magnifies out of all proportion the effect of a letter of May 12, 1869, from Marx to the National Labor Union in the U.S. Foner seriously suggests that it had a significant effect in calming rumors of war between England and America over the "Alabama Claim," the dispute over the prior sale of a British warship to the Confederacy. Serious and sympathetic attention to the radical complexities of international labor history we certainly need. This kind of hagiography, however, should by now be out of date.

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GORDON H. WARREN. *Fountain of Discontent: The Trent Affair and Freedom of the Seas*. Boston: Northeastern University Press. 1981. Pp. xiv, 301. \$18.95.

In these days of publishers' woes, it is not so common to find two books on the same subject being published within four years of each other. Inevitably, one is led to ask whether the work of Gordon H. Warren adds anything to Norman B. Ferris's *The Trent Affair: A Diplomatic Crisis* (1977). Unfortunately, the answer must be: nothing significant. A few unconvincing quibbles (couched in terms of lordly condescension) about Ferris's arguments in the footnotes of Warren's book are not enough to supply it with a *raison d'être*.

Warren does not always seem certain what kind of book he is writing. His highly enjoyable and gossipy pages on the capture of Mason and Slidell, written in a clear and lively style, would obviously appeal to the general reader but scarcely belong in a work of diplomatic history. Since he is unable to produce any new information, Warren is driven to embellish the story of the negotiations with a series of rambling digressions. There is a description of the funeral of the prince consort, and some verses from *Punch* on his death are reprinted; the reader is regaled with such tidbits of information as the fact that the Foreign Office was the only public department in Whitehall where employees were allowed to smoke or that Lord Palmerston spoke slowly to stop his false teeth from falling out (and like most American historians, Warren gets into a terrible muddle with British titles of nobility); there are sections on the Canadian militia, the Royal Navy's plans for an Anglo-American war, and the reaction of every government in Europe to the *Trent* affair. There is a chapter on international law, but its relevance to the subject of the book is cast into doubt by the author's admission that "for all the discussion, the *Trent* affair contributed little to the advancement of international law" (p. 198). All too often, the reader begins to wonder if Warren might not appropriately have paid a distant homage to Jean-Luc Godard and called his book: *The Trent Crisis: Two or Three Hundred Things I Know About Her*.

Amid all the minutiae really important questions are neglected. How intense was British indignation over the insult to their flag? A content analysis of the press would have helped here (especially if it included a sample of provincial publications). Music-hall turns can serve as a barometer of British working-class opinion in the nineteenth century, and Warren mentions a ditty about the *Trent* performed at Evans's: were there any more songs like this given at bigger halls, 1861 counterparts of "When They Bring Back the Flag to Majuba" or "By Jingo"? Warren describes the weakness of U.S. coastal defenses but fails to explain why the Lincoln administration remained oblivious to such considerations.

And the central mystery of the *Trent* crisis remains unsolved: after First Manassas, how could any intelligent American, let alone cabinet officers and members of Congress, view the prospect of simultaneous civil war and conflict with the world's only super-power with equanimity?

ADRIAN COOK
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JAMES M. SMALLWOOD. *Time of Hope, Time of Despair: Black Texans during Reconstruction*. (National University Publications Series in Ethnic Studies.) Port Washington, N.Y.: Kennikat Press. 1981. Pp. x, 202. \$17.50.

James M. Smallwood has examined carefully the prodigious primary materials related to Reconstruction in Texas, which include forty-four boxes of letters and one hundred seventy volumes of bound correspondence in the records of the Bureau of Refugees, Freedmen, and Abandoned Lands; the records of the Fifth Military District; material from the federal writers' project carried out for the Works Progress Administration in the late 1930s and early 1940s; and information from the American Missionary Association. The picture he draws from his exploration, however, does not much alter the contours of Reconstruction historiography of the last twenty years.

In a book of under two hundred pages, Smallwood surveys the state of life for slaves and free blacks in pre-Civil War Texas; the impact of the war itself; the imposition of peonage; the response of the black community in developing self-help movements and efforts to establish a system of education; and the politics that led to defeat of virtually all the aspirations and dreams of the black community. Despite the relatively small number of free blacks and slaves during the period, Smallwood indicates that there was little difference in the degree of harassment suffered by the black population in Texas compared with those of other states. The story is familiar: intimidation, violence, denied hopes, defeat, and ultimately despair.

Although Smallwood has provided a useful essay on a specific subject until now not investigated, his book suffers intellectually and linguistically from a too close connection to the doctoral dissertation on which it is based: that is, it is overcautious and unadventurous.

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PENELOPE L. BULLOCK. *The Afro-American Periodical Press, 1838-1909*. Baton Rouge: Louisiana State University Press. 1981. Pp. xiv, 330.

This book narrates the development of Afro-American magazines from the first such publication, *Mirror of Liberty* (1838–41), to the last initiated before *Crisis* in 1910 and the beginning of a “new era” (p. 1) in black magazines. Penelope L. Bullock discusses ninety-seven magazines whose primary editor or editors were Afro-American. She does not comment on such publications as newspapers and newsletters.

Bullock presents the magazines within their historical context, saying they proliferated when blacks needed them most. The first eleven publications emerged between 1838 and 1864 and were part of the struggle for emancipation. Only one magazine appeared between 1865 and 1879. During the Reconstruction period, Afro-Americans generally believed that the federal government would ensure their civil liberties and that they need not develop magazines to promote their views. Eighty-five black magazines were founded or proposed during post-Reconstruction, when previous expectations went unmet and Afro-Americans continued to suffer injustices by custom and law. Sixteen titles appeared in the 1880s, twenty-eight in the 1890s, and forty-one in the first decade of the twentieth century.

Although these periodicals were published across the nation, the greatest activity was in New York, with sixteen periodicals, and Pennsylvania, with fifteen. Most of the magazines had small readerships (a few hundred subscribers at most), limited funds, and short existences, usually not exceeding one or two years. Periodicals sponsored by organizations, particularly religious organizations, sometimes achieved more sustained publication.

This volume includes five chapters and three appendixes. They all provide useful information, but the most effective sections are chapter 3, “After Reconstruction: General Periodicals,” chapter 4, “After Reconstruction: Special Interest Periodicals,” and appendix A, “Publication Data and Selected Finding-List for the Periodicals.” In each of the chapters, Bullock surveys the magazines and then provides separate discussions of the more notable publications, such as *Colored American Magazine* (1900–09), *Voice of the Negro* (1904–07), *Medical and Surgical Observer* (1892–94), and *Negro Music Journal* (1902–03). Appendix A identifies the ninety-seven periodicals in alphabetical order, indicating for each magazine the title, date of first number, editor, circulation, and availability of extant issues, among other pertinent facts. Containing a wealth of detail, chapters 3 and 4 and appendix A particularly accomplish the purposes Bullock establishes for her book in the preface: that it give the reader “a broader knowledge of the history” of Afro-American periodicals, indicate source materials for researchers, and provide data identifying and locating the periodicals.

Although the volume is strong in narration and in

identification of source materials, it is not as successful in analysis. The detail is not consistently organized around a controlling thesis. Bullock states that the magazines represented “a cause” (p. 208), but she does not sufficiently compare their responses to that cause. She provides separate discussions for the more substantial publications, but she does not always explain why these particular magazines were more significant than their contemporaries. She bases her discussion on considerable research, but she does not cite the Booker T. Washington papers, a significant collection for researchers in the field and rich in information on the *Colored American Magazine* and *Voice of the Negro*.

In summation, emphasis is on the strengths of this book, for it makes a valuable scholarly contribution. The volume draws to a conclusion with a quotation from the *A. M. E. Church Review* (1884–present) on “the permanent and reference value” of each number (p. 205). Bullock identifies the ninety-seven magazines as important records of Afro-American history and culture. By energetically tracing their development, she makes this record available to her readers.

ABBY ARTHUR JOHNSON
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RONALD K. GOODENOW and ARTHUR O. WHITE, editors. *Education and the Rise of the New South*. Boston: G. K. Hall. 1981. Pp. xi, 303. \$19.95.

Because few comprehensive histories of Southern education have appeared since 1922, and because much valuable material in the field has been produced in the last few years, Ronald K. Goodenow and Arthur O. White have chosen to compile and edit *Education and the Rise of the New South*, a volume of essays they believe “will be of value to teachers, students, and researchers in the South and elsewhere” (p. vii). Arranged in chronological order and related generally to what the editors describe as the “New South phenomenon,” the essays deal with a wide variety of subjects. The impact of urbanization on public education, the endorsement by influential Northern liberals of Booker T. Washington’s accommodationist strategies, the introduction of progressive educational reforms into the rural South between 1900 and 1950, and various efforts in social engineering conducted during the Great Depression are among the areas investigated.

Some essays, like James Anderson’s “Ex-Slaves and the Rise of Universal Education in the New South, 1860–1880,” are revisionist. Anderson argues that freedmen in the immediate post-Civil War years, not Populists or Progressives in later decades, were the first native Southerners to “lay the foundations for universal education in the South”

(p. 18). Other essays, notably Wayne Urban's on educational reform in Atlanta's public schools and Joseph Newman's account of teacher unionism in the same archetypical New South city, break new ground. Urban shows that despite the appearance of significant curricular reform between 1890 and 1925, the forces of traditionalism and racism kept Atlanta's white public schools essentially unchanged. If that city's experience is not unique, he suggests, revisionist historians of educational reform may have overlooked an important point. "Only by paying attention to the opponents of reform, as well as to the reformers," writes Urban, "can historians arrive at a full understanding of the schools in their period of greatest change" (p. 128).

Biographical essays by Jennings Wagoner, Jr., and Mark Bauman also deserve special mention. Wagoner examines the personal and public contacts of Harvard president Charles Eliot and Booker T. Washington, concluding that by 1909 Eliot's (and other Northern liberals') endorsement of the Atlanta Compromise had effectively transformed it from a regional into a national formula for relations between the races. Bauman's study of Warren Candler, long-time president and chancellor of Emory University, is especially well written. A "genteel conservative" by nature and training, Candler had to confront the "New South Creed" head-on. His contributions to Southern higher education, for whites as well as blacks, were significant, despite his repugnance for the materialism that seemed to be undermining the values of an earlier age.

Although the narrow focus of some of the essays included in *Education and the Rise of the New South* may limit its appeal to professors and graduate students, the volume is a worthwhile addition to American educational history. The writing quality varies a great deal and the text contains a number of careless errors. But an excellent historiographical essay, contributed by Harvey Neufeldt and Clinton Allison, more than offsets these shortcomings. Until someone produces that long-awaited "comprehensive history" of Southern education, this collection of essays should go a long way toward filling the gap.

GERMAINE M. REED
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DANIEL E. SUTHERLAND. *Americans and Their Servants: Domestic Service in the United States from 1800 to 1920*. Baton Rouge: Louisiana State University Press. 1981. Pp. xv, 229.

Americans and Their Servants by Daniel E. Sutherland examines the history of an important, but often neglected aspect of America's past. Sutherland is concerned with relating the history of domestic

service in nineteenth-century America "in its broadest sense" (p. xiii). Throughout the book, the author endeavors to move beyond the stereotypes of servants as either saints or rascals and employers as either benevolent protectors or tyrants. From the outset, Sutherland makes it clear that the nineteenth-century "servant problem" entailed much more than a shortage of good servants. Rather, there was also a deficit of good employers, with both employers and servants expecting too much of each other. This situation, according to Sutherland, created an almost endless array of servant problems that were never adequately resolved.

Although Sutherland makes a well-intentioned effort to present a history of domestic service from the point of view of both the employer and the servant, one comes away from the book questioning whether the author ever truly arrived at an understanding of what service meant to the millions of people, mainly women, who had to resort to this type of work. Sutherland's continual reference to servants as "girls" is one indication of his lack of appreciation for his subjects. While he correctly points out that nineteenth-century Americans often referred to their servants as "girls," it is, nevertheless, inappropriate for the historian to adopt this nomenclature as his own. After all, the vast majority of domestic servants were grown women and deserve to be referred to as adult human beings.

It would have been useful if Sutherland had focused more specifically on the mistress-servant relationship in his chapter on employer-servant relations. While the author points out that masters were seldom seen, he avoids, for the most part, exploring many dimensions of the relationships that developed between women as mistresses and women as servants. It is especially regrettable that Sutherland did not probe this topic more fully since both employer and employee were almost always women. This relationship takes on even greater significance since it was one of the very few areas in which women had direct supervision over employees.

Sutherland concludes his study by arguing that the mechanization of housework during the early years of the twentieth century rendered domestic service unnecessary. With naive simplicity, he states that modern conveniences granted to American housekeepers "two wishes hitherto regarded as contradictory: a life free of both domestic drudgery and domestic servants" (p. 199). This rather shallow conclusion fails to take into consideration the research of Ruth Schwartz Cowan, David M. Katzman, and others who have shown that the decline of domestic service in America resulted in a shifting of housekeeping duties from the servant to the housewife herself. To suggest that mechanization miraculously freed American women (or men) from domestic drudgery is simply not accurate.

The subject matter of *Americans and Their Servants* closely parallels that of *Seven Days a Week: Women and Domestic Service in Industrializing America* (1978) by David M. Katzman. The Katzman book, however, provides a far better introduction to this complex subject.

JUDY BARRETT LITOFF
Bryant College

GLEND A RILEY. *Frontierswomen: The Iowa Experience*. Ames: Iowa State University Press. 1981. Pp. xv, 211. \$18.95.

JOANNA L. STRATTON. *Pioneer Women: Voices from the Kansas Frontier*. Introduction by ARTHUR M. SCHLESINGER, JR. New York: Simon and Schuster. 1981. Pp. 319.

These books are histories of the daily lives of women on the American frontier that describe their dreams and hopes, fears and deprivations, courage and stamina. Relying on the pioneer women's diaries, letters, journals, and memoirs, Glenda Riley and Joanna L. Stratton display the hardships and pleasures of pioneer life in Iowa from 1830 to 1870 and in Kansas from 1854 to 1890.

Until recently histories of the frontier have virtually ignored women. The frontier experience is viewed as a male stage with few appearances by women. When women are described, it is usually in a stereotyped or symbolic way—as "gentle tamers" who carried the culture and brought civilization to the West; as sun-bonneted wives and mothers who endured the hardships of frontier life as their husbands' helpmates; as prostitutes; or as hell-raisers like Calamity Jane, who were the women of the dime novels.

Some American literature gives the impression that the women's lives on the prairie frontier were nothing but endless drudgery that ground the women, as Hamlin Garland said, into battered ruins of their former selves. They were made coarse, ugly, and demoralized by the quality of their daily lives. In *The Wizard of Oz*, Dorothy's Aunt Em is portrayed as one of these women who was a pretty wife when she came to live in Kansas. But when we meet her, her eyes, cheeks, and lips are gray. She is thin and gaunt and never smiles.

Authors Riley and Stratton do detail pioneer women's struggles with their harsh winters; their dread of spring rains that caused their sod houses to leak and flooded their cabins; their fears of the prairie fires and tornadoes that would come suddenly and destroy all living things; their irritation with the insects, and especially the grasshoppers, which would strip vegetation and even green stripes off a little girl's dress; and their anguish over the

wolves howling outside their cabin doors. All these plagues and pestilences were realities and real fears for the pioneer women in Iowa and Kansas. In addition, both writers tell of the pain of leaving family and friends to begin the journey by prairie schooner, stage, or train, of the loneliness and isolation on the treeless homestead plains, and of the lack of privacy in the small frontier "soddies" and cabins. But they also show that the women demonstrated patience, persistence, endurance, and optimism.

The majority of the women came to the frontier as wives and mothers to "take up" the land with their husbands and help provide a better future for themselves and their children. Although the conditions were often harsh, they constantly looked to the future. Their records reveal that they were optimistic. They found joy in the growth of their families and homesteads, delight in the wonders of the cook stove or sewing machine, and comfort in the company of an occasional visitor or traveler.

Riley reports that the United States census listed the pioneer women as "not gainfully employed," but the record shows that they worked to provide homes, food, and clothing for their families and religious, educational, and political services for their communities. In addition to bearing and rearing children, they processed and prepared food, candles, fabric, and clothes and assisted with the farm labor. In their husbands' absences they often took over the farming and stock-raising operations. This was particularly true during the Civil War, when most of the Iowa and Kansas men were away fighting for the Union.

In the nineteenth century Iowa and Kansas were centers of support for the temperance and women's suffrage movements. Considerable detail is provided by the writers about the leaders and their activities on behalf of these social-reform movements. Stratton's book, however, is disappointing in that it tells us nothing new about women's participation in the Populist movement in Kansas.

These studies are primarily concerned with American, English-speaking Protestant women, who were the majority of pioneers in these areas. The authors mention women from other cultures, but they note the difficulty of locating information about black and Indian women. Likewise they fail to chronicle the lives of women who were prostitutes and camp followers.

Although these two studies of the second half of the nineteenth century deal with the heritage of pioneer women in Iowa and Kansas, they are very different books. Riley's is an analytical study in which she demonstrates that for every myth about frontier women there are "several levels of reality." Women's reactions to the pioneer experience, she says, are as varied as the women themselves. Strat-

ton's book is a descriptive narrative in which she skillfully uses the reminiscences of eight hundred Kansas women, which were gathered by her great-grandmother in the 1920s and preserved and organized by her grandmother.

These personal accounts of daily life show that one-dimensional interpretations and easy generalizations about women and their roles in the frontier experience will not suffice. Women are individuals who respond to the environment and the trials of pioneering in various ways.

Eminently readable, both books will be of interest to general readers as well as to professional historians. Overall they are excellent contributions to social history, to women's history, and to the history of the American frontier. One hopes that these publications will signal the beginning of regional studies that reach the realities of women's past and provide the basis for an understanding of women's role in the American westward movement.

BEVERLY BEETON
Governors State University

JACOB R. MARCUS. *The American Jewish Woman, 1654–1980*. New York: KTAV or American Jewish Archives, Cincinnati. 1981. Pp. xiv, 231. \$15.00.

JACOB R. MARCUS, editor. *The American Jewish Woman: A Documentary History*. New York: KTAV or American Jewish Archives, Cincinnati. 1981. Pp. xvii, 1047. \$35.00.

The doyen of American Jewish historians, Jacob Rader Marcus, has turned his considerable talents to the study of yet another neglected field, the history of American Jewish women. Marcus, now an octogenarian, has published two companion volumes, one a narrative history and the other a massive documentary of American Jewish women. Though complementary, the books diverge in their appeal. The history is a breezy account, unburdened with footnotes, of the lives of a variety of American Jewish women from the seventeenth century to the present decade. The documentary, while presenting letters and other writings by many of the same women mentioned in the narrative, is the first serious effort to bring before historians an impression of the rich material available for research on Jewish women's history in the United States. These sources will prove to be particularly valuable for future scholars who want to reconstruct the quality of Jewish family life, the religious attitudes of Jewish women, and the extent to which Jewish women absorbed the values of the American society surrounding them. The documents will enable students of American immigrant and Jewish history to develop a more complete understanding of the process of

ethnic group integration and to explore how being a woman shaped the contours of the ethnic experience. Despite his rather colloquial style, Marcus provides careful headnotes that situate the individual author of each document.

Like any new subject, *The American Jewish Woman* raises more questions than it answers. For example, in a suggestive paragraph Marcus briefly touches on the issue of why so few Jewish women participated in the burgeoning reform movements of antebellum America. Here he indicates both the inherently parochial attitude of most of these recent immigrants and the exceptional involvement of such a woman as Ernestine Rose, a discussion that helps prepare the background for a better appreciation of the reform impulse among Jewish women in the Progressive era. But Marcus's individualistic approach also limits the kinds of questions he can ask. The mass immigration of East European Jews to the United States begun in the 1880s requires techniques of social history to amplify and enrich the group portraits offered by Marcus. Needless to say, in a brief overview there is little space to analyze the internal dynamics of organizing Jewish women in the sweatshops, the rivalries among middle-class Jewish women's organizations, the tensions facing immigrant mothers and native-born daughters, or the relationship of feminism to the folk religion of Jewish immigrants. Similarly, given the constraints of a popular audience for which the book was written, Marcus adopts a tone of voice that undoubtedly will make feminist historians wince.

While I found the 1047 pages of the documentary uncomfortably heavy to carry or read anywhere except at a desk, I regretted the editorial decision to cut those texts that were well-crafted arguments in their original form, such as Paula E. Hyman's influential essay on women in the Jewish tradition. The publisher might have served the editor better by producing a two-volume documentary that would have let readers savor the full richness of the texts.

DEBORAH DASH MOORE
Vassar College

ESTELLE B. FREEDMAN. *Their Sisters' Keepers: Women's Prison Reform in America, 1830–1930*. (Women and Culture Series.) Ann Arbor: University of Michigan Press. 1981. Pp. viii, 248. \$18.50.

This history of women's prison reform explores new subject matter and provides a deeper understanding of the dismal record of prison reform in this country. Estelle B. Freedman's interpretation, however, goes beyond a narrowly defined institutional history. It contributes, as well, to the history of feminism and women. Divided into three parts,

Their Sisters' Keepers clearly distinguishes among generations of reformers and describes generational influences on the course of reform, from optimistic origins through ironic failures. Freedman artfully shows how the quest for purity, the prevailing sexual ideology, influenced the design of rehabilitation programs for women. She provides meaningful insights into the ideas of reformers, prison administrators, and women criminologists on controversial issues of the times, gives us a useful statistical profile of those sent to model reformatories in New York, Massachusetts, and Indiana, and, indirectly, addresses the relationship between women's "crimes" and culture. As a whole, this is a fine contribution to historical knowledge. Like any pioneering work, however, it has limitations, primarily of an interpretive nature.

A recurring problem stems from the implicit and incomplete way in which she describes feminist ideology. Following a brief comment on distinctions between political feminists and "social feminists," Freedman labors to distinguish feminine from feminist reform. Judgments about feminist thought, program, and rhetoric are scattered throughout the book, but the composite thrust of her interpretation is not always clear. Further, after the initial statements on feminist origins, I get little notion of a continuing interaction between prison reformers and the woman's movement that reshapes, or attempts to reshape, feminist ideology. Although transformations in the thought of prison administrators are examined, explanatory statements tend, too frequently, to be anchored in the personal and in personality.

At times, Freedman seems to be searching for feminist heroines. She contends that sisterhood, not class, motivated the first prison reformers. This judgment mechanically separates feminism from social class, which, in fact, is erroneous. Engaged in moral education for character building, moral educationists, inside and outside prisons, tended to take a maternalistic view of their young women. As a result, social class influenced their educational design, particularly in their advocacy of domestic education. This tension between class and sisterhood seems evident in reformatories even in the early years, but the author minimizes its presence and, occasionally, ignores it. Class and feminism are not mutually exclusive.

Assuredly, women criminologists' writings seemed moderate compared with the Lombroso hereditarians. Other contrasts might provide another and more complete basis for evaluation. How did these ideas compare with those of reformers who remained outside the professions? The 1910 New York Page Law controversy provided such an alternative reference point. Fourteen women's organizations mobilized against the law, protesting

neoregulation of prostitution—the states's involvement in the operation of commercial prostitution. Katherine Bement Davis, important in Freedman's history as a moderate criminologist, testified at a legislative hearing in favor of the Page bill, the only woman to do so. Later, after contact from the woman's movement, she repudiated the compulsory physical examination of suspected prostitutes, but took no position on fingerprinting, another target of feminist opposition. The point is that Davis's criminology was not written in a vacuum. It had a social context. Further, professionals and feminists did not always agree, as manifested during World War I when Davis, as Director, Social Hygiene Division, War Department Commission on Training Camp Activities, under the Chamberlain-Kahn Act, supported a program that repeated some of the worst portions of the Page Law. While British feminists vigorously combated an earlier version of the law, American feminists were more muted in their opposition. Thousands of American women were arrested and imprisoned under this act, which Freedman reported and deplored. Nevertheless, we get no sense of the role of prison reformers or feminists in—or attitudes toward—the war on prostitutes, despite the initial motive of prison reformers to rescue "fallen women." Since prostitutes may be considered a subclass or "lumpenproletariat," was a class factor present among some feminists on this issue? Was this war on prostitutes simply another manifestation of coerciveness, signs of which were appearing in women's prisons, as Freedman shows? Another issue, sterilization, became a focal point of reform criticism about the same time. We get no sense of prison reformers' attitudes, positive or negative, toward sterilization policies despite their primary impact on public institutions. Freedman's first citation on sterilization is a 1934 reference. Are these professionals isolated from developments in feminism? Do they act as an interest group? For whom did the criminologists write? As environmentalists who may have believed in inherited social characteristics, did they support eugenic programs?

Freedman did emphasize the tendency toward individual environmentalism in the remedial activities of prison reform. She is, however, less critical of the Institute of Social Hygiene, which pressed toward individual environmentalism, than I would be. John D. Rockefeller, Jr., Bedford's benefactor, usually did not support either people or projects by chance, as Freedman suggests. Regardless of the personal integrity of Katherine Davis, director at Bedford, we need to know how her research was used. It undermined traditional feminist views on the causes of prostitution and crime in a major way. In 1913, the American Social Hygiene Association, heavily supported by Rockefeller, published a front-page editorial against minimum wages for women as

a device to eliminate or, at least, curtail prostitution—a feminist preventive program. The editor cited the institute's findings—compiled by and under Davis's direction—to emphasize personal responsibility and moral choice over environmentalism. Ambiguities in Davis's research opened the data compiled at Bedford to alternative uses. A better sense of both the social context in which the ideas were formed and the social consequences of those ideas on the woman's movement and the greater society would be helpful.

Regardless of differences over and emphases in interpretation, Freedman makes a significant contribution to the history of American social reform in this monograph.

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JOHN C. SCHNEIDER. *Detroit and the Problem of Order, 1830–1880: A Geography of Crime, Riot, and Policing*. Lincoln: University of Nebraska Press. 1980. Pp. xiv, 171. \$13.50.

John C. Schneider has provided historians of law enforcement with another book on police reform in the nineteenth century, but this is a case study with an interesting difference. While acknowledging the role of ideology, politics, and ethnicity in the emergence of professional police departments, he concentrates on the physical context in which reform occurred. In Schneider's view, Detroit's spatial arrangements influenced not only the types and locations of crime and violence but also the timing and dispositions of police reform.

In the process of demonstrating his thesis, Schneider provides a fascinating analysis of the interaction between urban geography and urban society. He traces the roots of rioting against brothels during the 1850s to the conflict between working-class ethnic communities and vice entrepreneurs for control of urban space. In another chapter he offers one of the most careful descriptions we have of the growth of downtown vice districts. He also discusses the locations and composition of a bachelor subculture, a hitherto neglected topic that Schneider rightly considers one of the most crucial features of nineteenth-century urban life.

By the 1860s these social developments had produced a spatial configuration in which the location and level of criminal activity was fairly predictable. Crime was concentrated in the emerging downtown, a fact that provoked a police reform movement led by Detroit's upper classes who felt that their homes and businesses were in jeopardy. Once in place, the city's police became instruments for the control of

urban space, most especially the spaces occupied by fashionable neighborhoods and the downtown.

Schneider's account of these developments is generally well done. There are, however, a few problems worth considering. By focusing on vice areas that catered overwhelmingly to lower-class and bachelor residents, he overlooks high-class gambling houses and brothels. Schneider therefore ignores out-of-town businessmen—an important source of customers for the vice districts in other cities. Those businessmen, and other visitors, would have been worth discussing since their housing, eating, and leisure needs affected the spatial development of downtowns in Chicago, New York, Philadelphia, and, perhaps, Detroit as well.

Although Schneider found little conflict between patrolmen and reformers over the goals and methods of law enforcement, he does offer some intriguing, though limited, evidence that some conflict did exist. He mentions that patrolmen resorted to the use of firearms too frequently in the 1870s. Those incidents may be important clues to the development of a police subculture, one of the most neglected topics in historical studies of policing.

These caveats aside, Schneider has written a carefully argued work that offers a theoretically interesting and important framework for the further analysis of nineteenth-century policing.

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LAWRENCE M. FRIEDMAN and ROBERT V. PERCIVAL. *The Roots of Justice: Crime and Punishment in Alameda County, California, 1870–1910*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1981. Pp. xiii, 335. \$27.50.

Anecdotal books on crime and punishment in American life abound. The serious examination of the history of criminal justice, however, is a relatively recent development. Thoughtful scholars, employing the tools and illumination of the new social history, have just begun to mine the rich lode of materials on crime in this country's past. *The Roots of Justice: Crime and Punishment in Alameda County, California, 1870–1910* by a well-known legal historian, Lawrence M. Friedman, and his attorney-collaborator, Robert V. Percival, is clearly one of the most valuable studies fashioned from this recently exposed vein of American lore.

The Roots of Justice is, as its authors claim, the first thorough study of the incidence of crime, the patterns of arrest and indictment, the decisions of criminal courts, and the practices of prisons in a single American locality. In short, this is the first examination of the actual operation of an entire

system of criminal justice. As such, this maiden effort deserves considerable—although not unmitigated—praise.

Friedman and Percival have placed Alameda County under their legal microscope. Alameda's principal city and the center of its criminal justice system is Oakland, a blue-collar brother to its dazzling older sister, San Francisco, just across the bay. Most of the sources examined by the authors are from Oakland: the original police arrest blotters, the Superior Court records, and the pages of the *Oakland Tribune*. The most important findings are presented in seventy-one statistical tables scattered throughout the book. It is impossible in a brief review to do more than hint at the richness and variety of the authors' conclusions. For example, Friedman and Percival found that the incidence of criminal activity, adjusted to population, actually declined as Oakland grew larger and more industrialized in the late nineteenth century. The most common types of crime were found to be drunkenness and brawling, but the most frequently occurring felonies were related to property and not acts of violence. Not surprisingly, the authors' statistical compilations revealed that the "dangerous class" was disproportionately composed of society's most disadvantaged groups—drifters, transients, and racial minorities.

Although the sheer number of statistics in *The Roots of Justice* will no doubt overwhelm some readers, quantitative historians will be distressed that the authors have done little more than sample, count, and record percentages; for whatever reason, sophisticated quantitative techniques have not been employed. The reader looking for tidbits of criminal miscellany will, however, not be disappointed. Friedman and Percival have reported hundreds of pieces of anecdotal material, including this harangue by a county prosecutor: "If you can transform a rattlesnake into a human being, you would still have a man better than Smith's murderer, because at least his rattles would give a warning" (p. 186). A chapter on the headline-making trials of the period, trials that the authors argue served to provide moral lessons to the curious Victorians of Alameda County, is replete with revealing details of the foibles and fantasies of the period.

Frequently, scholars engaged in producing "histories without names" pay more attention to numerical exactness than precise use of language. Overall, Friedman and Percival's book is better written than most social histories. It contains a number of hackneyed phrases, however, ("Alameda County's heart beats with a mighty pulse" [p. 20]; "plea bargaining was less common, by a county mile" [p. 176]), some mixed metaphors ("paint the system as an engine of oppression" [p. 13]), and several unnecessary internal signals ("We will return to this problem again;

for now we note that" [p. 148]; "We return to this later on" [p. 150]).

Notwithstanding the criticisms expressed here, *The Roots of Justice* is a first-rate addition to the growing literature on the history of criminal justice. One of its major strengths is its fidelity to the limits of the Alameda experience. Although the authors occasionally compare late nineteenth-century crime in their California county to criminal activity elsewhere and at other times, they avoid facile generalizations about "crime and American history." Friedman and Percival have produced a pathbreaking study; other students of crime and punishment would be wise to undertake similarly ambitious local expeditions.

JOHN W. JOHNSON
Clemson University

MARY YEAGER. *Competition and Regulation: The Development of Oligopoly in the Meat Packing Industry*. (Industrial Development and the Social Fabric, number 2.) Greenwich, Conn.: JAI Press. 1981. Pp. xxvi, 296. \$30.00.

One is tempted to begin this review by noting that Mary Yeager has written a meaty book on an important subject, a study certain to beef up the existing literature. Indeed, she points out that the only "wide ranging history" on the meat packing industry dates from 1923. Why this paucity of historical scholarship about an industry that was clearly oligopolistic, highly creative in its use of new administrative techniques and competitive patterns, and at the same time an obvious target for governmental and public pressure against big business? The author hints at the problem when she writes that a combination of public antagonism and corporate defensiveness "effectively sealed the archives of the major meat packing firms." Yeager began this work some years ago as a dissertation at Johns Hopkins University. Apparently for that version and this published volume, only one firm (Swift and Company) provided material, and that was of a very limited nature. The refusal of large corporations to open their archives for the purposes of dispassionate scholarship while explicable remains inexcusable. It has not, however, prevented Yeager from producing an impressive study, one that builds upon the previous work of scholars such as Alfred Chandler, Jr., Louis Galambos, and Alfred Eichner.

Why, Yeager asks, did oligopoly come to characterize American central industrial structure? Why did it precede monopoly in certain cases (such as meat packing), and follow it in others? The author answers these questions through examination of the meat packing industry as a case study. Her central thesis appears to be most clearly stated in a footnote to her fifth chapter. Conceding that oligopolistic

competition often brought an end to "classic" competition based on prices. Yeager rejects the tendency to associate oligopoly with a decline of competition. She argues, to the contrary, that its nature changed, "and that oligopoly meant the emergence of a new type of competition, based less on prices than on considerations such as product, quality, and service." Rather than disappearing, competition merely changed form (p. 128).

Yeager traces the emergence of the "big four," Swift, Armour, Morris, and Hammond. She describes their common tendencies toward both vertical integration and extensive by-product manufacture. Indeed, by the 1890s Mr. Dooley claimed that "a cow goes lowin' softly into Armour's an' comes out glue, gelatine, fertylizer, celooloid, joolry, sofycusions, hair restorer, washin' sody, littrachoor an' bed springs so quick that while aft she's still cow, for'ward she may be anything fr'm buttons to pannyma hats" (p. 68).

The author also explores the importance of technological innovation, especially the refrigerator car, in the bitter competition between the older livestock shippers and the newer dressed-beef interests. Caught in between, of course, were the railroads who vainly sought some sort of "neutral" policy acceptable to both. "Guided by an entrepreneurial aggressiveness that thrived in a profit-oriented economy, competition and technology disrupted the railroads' search for order, and thrust a few large, integrated multifunctional meat packing enterprises into the center economy" (p. 105).

Yeager concludes that unlike other oligopolies such as sugar or oil, "in the dressed beef industry vertical integration and oligopoly preceded horizontal combination and monopoly" (p. 242). This took place because the overriding problem for the packers was marketing, not overproduction. Solution to this problem lay along the lines of price leadership, rather than simple competition. Cooperation became an important part of oligopolistic competition. The origins of oligopoly differed in other industries, but there were two causal patterns that seemed clear: (1) the need to overcome marketing problems, and (2) the challenge to control overproduction. The actions taken regarding the first area remain crucial in explaining the history of the dressed-beef industry.

Although guilty of the forgivable tendency of including important material in footnotes that ought to be elevated to the text, Yeager has produced a valuable contribution to our understanding of the dynamics between markets and technology. Her study is, as she hoped in her preface, well worth "at least three hours of your time."

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ROBERT D. PARMET. *Labor and Immigration in Industrial America*. (Immigrant Heritage of America Series.) Boston: Twayne. 1981. Pp. 268. \$15.95.

Robert D. Parmet's *Labor and Immigration in Industrial America* may prove more useful in its parts than as a whole. This is because the book does not have a readily discernible argument and has only a most amorphous general purpose, to present "a clear picture of the often strained relationship between American and immigrant workers in the era of industrialization and mass immigration" through an emphasis on "ethnic relationships and their effect on labor unions and American immigration policy."

Many of the chapters of this awkwardly organized book are clearly written essays summarizing some of the most important recent scholarship on particular ethnic groups in the industrial work force of the late nineteenth and early twentieth centuries. While it is by no means clear what criteria Parmet used in selecting the subjects for the first eight of his twelve chapters, students would find here a good handle on the literature for the Chinese, the steel mill East Europeans, the polygot coal and garment industry workers, and Italian workers more generally. There is only an elusive connection between these essays and the final four chapters, where Parmet presents a traditional account of the labor movement's participation in the immigration policy debate culminating in the Immigration Act of 1924.

At the risk of putting words into Parmet's mouth, the implicit argument seems to be that despite the fact that immigrant workers demonstrated some notable loyalty to unions, the labor movement's coming to terms with American capitalism logically led it to translate its experience of ethnic competition into a hostility to further immigration. Unfortunately there is only the briefest suggestion in the end that this may be the thesis, and on the whole other treatments such as Philip Taylor's *The Distant Magnet* and Alan M. Kraut's *The Huddled Masses* are more satisfying general introductions to the subject of immigrant workers in this period.

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GEORGE R. NIELSEN. *The Danish Americans*. (Immigrant Heritage of America Series.) Boston: Twayne. 1981. Pp. 237. \$14.95.

The generally accepted view of rapid Danish assimilation, quicker, and presumably less painful, than that for any other nationality, introduces this short study of the Danish-American community. Lacking any sociohistorical description of the absorption of Danish immigrants into American society,

George R. Nielsen nevertheless, and only with scant reference to supporting statistics on rate of intermarriage and on naturalization and English language proficiency, delves into an array of speculative explanations, finally suggesting that whereas internal discord and strife among Norwegian immigrants tended to unite them and prolong group solidarity, similar conditions among the Danes had the opposite effect, destroying ethnic cohesion due to the weakness of Danish-American institutions.

In succeeding chapters the case for Danish uniqueness is considerably compromised by the evidence of viable community life among Danish Americans. In Chicago, for instance, by 1900 a thriving urban colony of ten thousand had formed the basis for separate social and cultural institutions, and one of the two Danish-language newspapers still in existence in this country, *Den Danske Pioneer*, is currently published in that city. In comparison to the Norwegians—often thought of as the most ecologically segregated of the Scandinavians—and the Swedes who have been less so, Danish immigration has, however, evinced several differences. Danes emigrated somewhat later, in much smaller numbers (between 1840 and 1914; about 309,000), and in the United States dispersed more widely and formed few colonies.

Religious dissension, well described and analyzed by Nielsen, derived from tensions within the Lutheran church in Denmark, and among the immigrants re-emerged as two competing Lutheran church bodies, one of which expressed the happy Christianity of N. F. S. Grundtvig and for periods of time was responsible for no fewer than five folk high schools in the national and liberal Grundtvigian tradition. The reforms embodied in the Danish constitution of 1849 encouraged dissatisfaction with the ineffective Lutheran church and made Danes receptive to missionary efforts by religious dissenters, such as the Mormons, who made many converts in Denmark and caused a substantial Danish exodus to Utah. Nielsen also devotes much space to explaining the Danish socialists, their unsuccessful colony on the plains of Kansas, and their cooperation with other Scandinavian socialists in Chicago.

The "Immigrant Heritage of America Series," to which this volume belongs, has endeavored to map the state of scholarship on America's multiple ethnic groups. The Danish-American community has received less than its share of scholarly attention. Nielsen's treatment, although the organizing principle he has chosen creates a somewhat disjointed impression, is a helpful guide, touching on and interpreting the main aspects of a Danish presence in America.

ODD S. LOVOLL
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URI D. HERSCHER. *Jewish Agricultural Utopias in America, 1880–1910*. Detroit: Wayne State University Press. 1981. Pp. 197. \$15.95.

Uri D. Herscher's brief essay retells the saga of the agricultural colonies founded by and for East European Jewish immigrants to America at the close of the nineteenth century. Fired with the ideal of regenerating themselves—creating a new "productive" Jew on America's soil—Russian Jewish radicals, members of *Am Olam* (Eternal People), ventured to such remote settings as Sicily Island, Louisiana, New Odessa, Oregon, and even to an unnamed tract of land ninety miles northeast of Little Rock, Arkansas. Farm efforts also were initiated by various Western European Jewish relief funds for their immigrant clients. In their zeal to see East European Jews settle anywhere but overcrowded New York, organizations like the French Alliance Israelite Universelle looked to Bethlehem-Jehudah and Cremieux, South Dakota, as potential new homes for those fleeing tsarist Russia. Indeed, Herscher's work suggests that the philanthropists' commitment was so intense that these politically conservative leaders apparently had few qualms about bankrolling even the most radical *Am Olam* project.

Each of these colonies quickly or eventually failed. Most were poorly conceived and administered by Jews with little prior husbandry training. Some were defeated by heat, floods, malaria, and mosquitoes, and all suffered from their creation at a time when the tide of American demography and economy was sweeping from the countryside to the industrializing cities. Within days, months, or a few years, dispirited settlers rejoined the masses of their brethren within the urban frontier. Significantly, Herscher notes that the only adventures that proved relatively successful were the agro-industrial colonies of southern New Jersey set not too far from the social-industrial hubs of New York City and Philadelphia.

Despite their "record of failure," Herscher claims, their story is a poignant expression "of the yearnings to which East European Jewish idealists gave themselves" (p. 31). Unfortunately, this present narrative fails to evoke the required empathy for their struggle. Descriptions of trials and disappointments are derived primarily from emotionless early histories of the sponsoring philanthropic organizations. The fears and unfulfilled expectations of the participants themselves, obtainable only from ephemeral memoirs and personal correspondences, remain basically unexplored. One would like to read, for example, about the genesis of their belief system and their sense of loneliness and separation from the centers of Jewish life. And one needs to know both of their relationship to their patrons and of their hopes and plans as the experiments failed. To be sure, the author is undoubtedly well aware of

this essential lacuna as witnessed by his publication of an important, long, extant memoir from a survivor of the New Jersey experience as an appendix to the study. But still more needs to be known about what drove these idealists for historians to care about ventures that are themselves only footnotes to Jewish immigration history.

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RONALD E. MARTIN. *American Literature and the Universe of Force*. Durham, N.C.: Duke University Press. 1981. Pp. xviii, 284. \$27.75.

Assessing the troublesome world of 1812 in his *History of the United States*, Henry Adams concluded that all the celebrated actors were helpless pawns in a drama of cosmic proportions, men who "followed or resisted at haphazard the necessities of a movement which they could not control or comprehend." Although the prose was vintage Adams, the imagery was not uniquely his. References to cosmic necessity were common in Gilded Age thought, a stock element in what Ronald E. Martin has called "the universe of force."

Heirs alike of the enlightened belief in natural law and the romantic vision of an omnipotent world spirit, many nineteenth-century thinkers attributed the workings of material reality to a surging, ever-present universal force. Both the conservation of energy principle and the theory of evolution were manifestations of force thinking. But it was Herbert Spencer who explored the fuller implications of force mechanics, pursuing the leaven of force into every area of human thought.

In post-Civil War America Spencer's philosophy received an enthusiastic welcome, for it offered intellectual moorings to a people awash with change and uncertainty. By the end of the century, however, cracks had begun to appear in the Spencerian synthesis. Scientists as well as philosophers rejected natural law and cosmic necessity in favor of functional probabilities; scientific theories were now convenient mental constructs. Yet force concepts had so pervaded the American mind that repudiation by the scientific community made scarcely a ripple on the public consciousness. And this lag was nowhere more evident than in American literature. Only Henry Adams, among the four authors whose works Martin discusses, appreciated the abandonment of force thinking, and he took his revenge by citing that abandonment as one more proof of social devolution.

For the other three writers, Frank Norris, Jack London, and Theodore Dreiser, the universe of force remained a very real and effective literary mechanism. In Norris's novels it was the primordial

force of animal instinct that accounted for the behavior of his characters and linked them together in a causal network. Norris unified such instincts further in his wheat trilogy by subsuming them in the remorseless power of grain as it sliced through the soil and enfolded the fortunes of mankind in its unfeeling grip. More sophisticated in his use of force philosophy, London effectively pitted the determined individual against equally stubborn natural and social requirements. Dreiser compounded the force equation with a welter of conflicting individual wills, the dynamics of which were again deflected by the demands of natural law and social convention. Despite the limitations of such a philosophical approach to the novel, Dreiser's writings transcended his antiquated intellectual framework. Force concepts and images provided Dreiser and fellow novelists with powerful insights into the human condition.

In analyzing the debt of these authors to the universe of force, Martin has produced a masterful, well-written study of American literature and its relation to a particularly rich phase of Western thought. At times the author is driven by his knowledge and enthusiasm into unnecessary detail, but such excesses do not detract seriously from a book that will long remain essential reading for the student of American literary and intellectual history.

DAVID R. CONTOSTA
Chestnut Hill College

T. J. JACKSON LEARS. *No Place of Grace: Antimodernism and the Transformation of American Culture, 1880-1920*. New York: Pantheon. 1981. Pp. xx, 375. \$18.50.

This is a powerful and provocative reinterpretation by a young historian of the dominant Anglo-American culture of the late nineteenth and early twentieth centuries. It is a book that all scholars in the field will have to take into account. T. J. Jackson Lears's central thesis is that the Anglo-American bourgeoisie experienced a profound psychological trauma as corporate capitalism replaced entrepreneurial capitalism. He believes that the cultural responses to this trauma were much more complex and much more significant than was understood by an earlier generation of historians as represented by Richard Hofstadter. The complexity of responses came from overt criticism by the middle class of the emerging world of impersonal bureaucracy, which covertly made it possible for Americans to adjust to that world. For Lears, however, the covert activity was not self-conscious or conspiratorial but sprang from a deep psychological ambivalence, which characterized the personalities of many Anglo-American cultural leaders. The ironic significance of the criticism

was that it served in the short run to revitalize the Anglo-American middle class but in the long run constructed the theoretical foundation for much of the subsequent criticism of the bureaucratic state and the politics of limitless economic growth. Lears believes that many of the current ecological and economic voices that are proposing limits to growth, as well as political decentralization, are speaking from positions established by men such as Henry Adams.

He believes that the pressures on the Protestant autonomous self to conform to the impersonal values of corporate bureaucracy caused a desperate search for a more authentic personal experience. This search led many Anglo-Americans to look to the medieval past or to the Orient for cultures that did not demand the suppression of so much of the individual's personality. But in his chapters describing the attraction of the sense of honor, craftsmanship, art, and ritual in these exotic cultures, he develops the irony that only fragments from these cultures were used by individuals to ease their private pain in accepting bureaucratic discipline. This, in part, is what he means when he describes the transition from a Protestant to a therapeutic culture. Instead of advocating a whole different culture, Anglo-Americans, by finding personal solace, facilitated the transition from a producer-oriented to a consumer-oriented economy.

He explores in depth a number of individuals such as Charles Eliot Norton in order to demonstrate the way in which larger cultural patterns had their origins in family history. He argues that the therapeutic turn toward medievalism or Orientalism expressed a questioning of the male world of authority and a desire to free the more "feminine" side of the male personality. He concludes with a chapter on Henry Adams who, according to Lears, broke from the unresolved ambivalence of men like Norton by rejecting their acceptance of a public life that supported corporate capitalism and its masculine ethic of the domination of nature, an ethic of becoming, while searching in their private lives for a "feminine" ethic of being, an ethic symbolized by medievalism or Orientalism.

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ANN L. HOLLICK. *U.S. Foreign Policy and the Law of the Sea*. Princeton: Princeton University Press. 1981. Pp. xii, 496. Cloth \$32.50, paper \$15.00.

JOHN W. COOGAN. *The End of Neutrality: The United States, Britain, and Maritime Rights, 1899–1915*. Ithaca, N.Y.: Cornell University Press. 1981. Pp. 284. \$19.50.

The End of Neutrality, by John W. Coogan, is a compact, carefully reasoned, and well-documented study of the politics of maritime rights in United States–British relations with a focus on the years 1819 to 1915. The book begins with a review of American policy regarding maritime rights, neutrality, and freedom of the seas from the administrations of Washington and Jefferson and concludes with a commentary on the implications of United States entry into the First World War in 1917. Coogan has carefully examined the longstanding thesis that the United States, led by Woodrow Wilson, pursued a policy of even-handed neutrality in the years before American entry into the war against Germany. Having looked carefully and in detail at the evidence of what was actually done in Washington and London during these years, and putting the public oratory in perspective, Coogan arrives at what some readers may regard as a revisionist conclusion. Without appearing to change its traditional posture, the United States, under the leadership of Wilson, moved from a largely neutral position in relation to the European conflict into a neutrality benevolent with respect to Britain and finally to outright alliance.

In this transition did Wilson deceive the American people, and did he deceive himself? Coogan appears to believe that he did both. Wilson and many of his closest associates had a strong emotional attachment to Britain and the British cause, but Wilson also had a vision of moral universalism that he hoped would replace the national state balance of power as a foundation for world politics. This combination of biased neutrality and moralistic idealism contributed to a very different outcome in international affairs than that which Wilson encouraged. These conclusions are hardly novel or revisionist and come through clearly in Edward H. Buehrig's essay, "Woodrow Wilson and Collective Security," in the volume that he edited entitled *Wilson's Foreign Policy in Perspective* (1957). World War I ended in an incredible contradiction. On the one hand the treaties of Versailles and Lausanne confirmed the worst tendencies of the international order of the nineteenth century, and, on the other hand, the creation of the doomed-to-failure League of Nations was established upon assumptions that were totally inconsistent with the existing political state of the world. An implicit question that may be left in the minds of many readers of Coogan's book is whether the United States is capable of having a rational and farsighted foreign policy. The tendency of nations generation after generation to repeat their mistakes is more impressive than their willingness and perhaps capacity to learn from historical experience.

Ann L. Hollick has long been recognized as one

of the leading students of ocean policy. Her book, *U.S. Foreign Policy and the Law of the Sea*, is the result of four years of work on the evolution of United States maritime policy during the quarter-century following the end of World War II. This book, and the one by Coogan, might both be read as case studies in what should and should not be learned from history as a guide to wisdom in policy. Both books underscore the almost insuperable barriers to consistency and coherence in the foreign policy of a highly pluralistic democracy such as the United States. Both books have the great merit of an effective integration of micro- and macrolevel analysis. Hollick examines American ocean policy in terms of technological imperatives and the nation's geographic situation and resource needs; her analysis draws upon American political psychology involving the polar attractions of isolationism and internationalism and the changing role of the United States in the prevailing international system. But she observes: "While it is important to be attentive to these broad levels of explanation, they do not offer, in and of themselves, a sufficient understanding of national ocean policy. The analyst must supplement the macro-approaches with a micro-level analysis focusing on the decision-making or policy process" (p. 12).

Following a brief summation of United States ocean policy as it stood prior to World War II, Hollick focuses on the specific issues of fishing rights, the continental shelf, and primarily on the successive United Nations conferences on the law of the sea, dealing in one chapter with the first and second UN conferences of 1958 and 1960, which were relatively limited and conclusive. Roughly the last third of the book describes the steps leading toward the third UN conference on the law of the sea and analyzes the causes and implications of its inconclusive results.

The effectiveness with which she has balanced micro- and macroanalysis and her broad understanding of the dynamics of ocean policy give the book a quality of constructive realism often absent from writings on international affairs.

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JEFFERY M. DORWART. *The Office of Naval Intelligence: The Birth of America's First Intelligence Agency, 1865-1918*. Annapolis: Naval Institute Press, 1979. Pp. ix, 173.

Nearly forty years ago Seward W. Livermore first explored the records of the Office of Naval Intelligence to reveal their promise for the study of naval

diplomacy during the early twentieth century. Yet these rich materials, perhaps in part because of their quantity and complexity, remained largely unworked before Jeffery M. Dorwart undertook this brief, succinct history of ONI from its founding in 1882 to the close of World War I. Dorwart's study is in large part a chronological survey of the naval intelligence chiefs and the problems they faced.

The Office of Intelligence was established as part of the movement to secure the advanced technology necessary for construction of the then "New Navy" of steam, steel, breechloading guns, and electricity. The first officers drawn into naval intelligence were inspired by euphoria arising from their sense that they were creating one of the principal elements of a naval general staff. Although the usefulness of naval intelligence was early recognized by significant persons both inside and outside the navy, the naval intelligence directors remained second-level bureaucrats, never becoming significant molders of high policy. Naval intelligence also never became a career service, and naval intelligence duty alone was clearly not a recommendation for good commands elsewhere in the navy.

Dorwart deals briefly but knowingly with a myriad of problems and personalities significant to ONI during its first thirty-six years. Whereas the office initially dispatched attachés to Europe in search of advanced technology, its officers by the 1890s were planning for operations against Britain (Canada), Chile, and Spain. The war with Spain witnessed marked, if somewhat crude extension of naval intelligence undercover activities designed to learn Spain's intentions. As did the Naval War College and the new General Board, ONI focused during the early years of the twentieth century on the rising naval power of Germany and Japan, even as it was drawn into such tangential activities as the "dollar diplomacy" of the Taft administration. It is Dorwart's view that the thrust of ONI altered significantly after 1912 as sources of information were progressively closed to its attachés abroad and as the office moved into a variety of new, often clandestine activities. About a third of Dorwart's study deals with ONI during World War I: its role in the new Office of Naval Operations, its efforts to establish intimate relations with British intelligence, its rivalry with Admiral Sims's intelligence group in London, its proliferating surveillance of subversive acts, its growing bureaucracy, and much more. At one point, Admiral William S. Benson, the chief of naval operations, was obliged to remind ONI that its first responsibility was to provide information to the navy. In due course, ONI's more peripheral activities were drastically curtailed by severe budget cuts after World War I.

Dorwart's achievement has not dispelled a persist-

ent sense on the part of this reviewer that the known ONI records before 1918 are incomplete and thus inadequate to tell the whole story. The National Archives retains very full naval intelligence general correspondence and attaché reports that seem to reflect the scope of ONI's interests to 1912. Thereafter, the attaché reports or registers persist in great volume while the general correspondence series terminates. The open intelligence files for the period after 1912 deal with many of the topics handled by Dorwart, but they do not contain the running correspondence that the naval intelligence directors presumably maintained with the attachés abroad. Nor do the files include estimates of the situation that ONI surely prepared from reports then reaching Washington in increasing volume. In short, there may be ONI records still to be discovered that relate ONI more directly to the formulation of naval policy and reflect more clearly ONI's views on the international naval situation. Nevertheless, Dorwart manages from the mass of naval intelligence material already available to complete an informative and wholly original study in naval institutional history.

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REGINALD HORSMAN. *Race and Manifest Destiny: The Origins of American Racial Anglo-Saxonism*. Cambridge: Harvard University Press. 1981. Pp. 367. \$22.50.

During the first half of the nineteenth century, Reginald Horsman maintains, American intellectuals developed an Anglo-Saxon racist ideology that was readily accepted by most of their countrymen. It postulated an American national character as the purest exemplification of innate Anglo-Saxon and Teutonic traits. Inherently superior to blacks, Mexicans, Indians, and even non-English white immigrants, white Anglo-Americans allegedly benefited mankind by "elevating," exploiting, and even destroying these "lesser" races. To some extent, this justification for Indian removal, black bondage, nativism, and appropriation of Mexican lands was rooted in European "scientific" thought. But it also derived from the unique American experience of westward expansion and continental conquest.

Horsman buttresses this theme with an abundance of primary sources, graceful writing, and sensible organization. He proves what historians like George M. Fredrickson and Ronald T. Takaki have suggested—that racist "Anglo-Saxonism" had become both a fully developed intellectual construct and a widely accepted ideological perspective by the end of the Mexican War. A dehumanizing way of looking at other peoples and cultures, it had taken the place of a much more limited notion propound-

ed by the Founding Fathers—the proud Anglo-Saxon origins of English and American political institutions. Substituting for the more cosmopolitan attitudes of the Founding Fathers, racist "Anglo-Saxonism" contoured American thought well before the arrival of the "new immigrants" at the end of the nineteenth century.

Unfortunately, this very important study suffers from two serious deficiencies. First, Horsman offers no developed explanation of why "Anglo-Saxonist" racism became ascendant when it did. At times, he implies what he states explicitly at the end of the study—that it satisfied "the need to justify exploitation and destruction" of nonwhites and immigrants (p. 300). But suggestions and a last minute assertion are no substitutes for sustained argumentation. Second, the volume is insufficiently attentive to emotional or inner motivational differences between proponents of "Anglo-Saxonism." To be sure, Horsman notes that a number of Northeastern Whigs and abolitionists did not fully subscribe to the concept. But he fails to explore systematically different levels of emotional attachment to the concept among the overwhelming majority of whites who accepted its premises. Emerson and Calhoun both expounded "Anglo-Saxonism," for example, but from vastly different levels of emotional intensity owing to significantly different inner needs. These inner differences explain why the two men behaved quite differently toward nonwhites.

Race and Manifest Destiny is tarnished in these two respects because Horsman places harsh limitations on the scope of his inquiry. He chooses to focus almost entirely on intellectual history—"how the ideas of various sections of the intellectual community both reflected and influenced popular and political attitudes." At the same time, he consciously eschews considering how "Anglo-Saxonism" advanced "the influence of various classes and groups" or how it sustained their inner emotional needs (p. 6). By screening out economic and political ambitions and other, more subtle, inner motivations from the scope of his analysis, Horsman deprives himself of the resources that he needs to help students of the early national period with perhaps their foremost interpretive problem. He adds little to efforts by historians like Marvin Meyers and Linda K. Kerber at explaining *why* the relatively benign attitudes of the founding fathers toward other peoples and societies were replaced by the dogmatic nationalism and the coarse bigotry of their sons and grandsons.

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GERALD W. PATTON. *War and Race: The Black Officer in the American Military, 1915-1941*. (Contributions in

Afro-American and African Studies, number 62.) Westport, Conn.: Greenwood Press, 1981. Pp. x, 214. \$25.00.

It is a story all too familiar to historians of the black experience in the United States. Blacks are excluded from participation in some part of American society. When they grudgingly receive minimal entry, their activities are ignored or falsified into a generalized condemnation. The distorted conclusion is then used to justify past, present, and future discrimination. In this book, which was originally a 1978 doctoral dissertation at the University of Iowa, Gerald W. Patton of Washington University, St. Louis, discusses another example of this distressing story. This one concerns the fate of black officers in the U.S. Army during World War I and the pre-World War II period.

Patton concentrates his efforts on the black attempt to join the army officer corps during World War I. He details the successful campaign of the Central Committee of Negro College Men and others to establish a separate officer training camp for blacks at Des Moines, Iowa. This victory proved to be Pyrrhic, however, because the military establishment continued to discriminate against blacks and foiled their efforts, once commissioned, to become an integral part of the army. Particularly revealing is Patton's discussion of the army's distortion of the World War I combat performance of seven hundred black officers. These inaccuracies were later incorporated into several key army reports and were then used to justify virtual elimination of the black presence in the pre-World War II military. Despite claims to the contrary, military officials used widely accepted racial stereotypes, not facts, as the basis for their antiblack actions.

While this book is an accurate synopsis of the fate of the black officer during the period in question, it omits a great deal that would have explained this fate more fully. The black common soldier is mentioned only in passing, although an examination of his military experiences would have shed much light on the problems of black officers. The reaction of the French military and civilians is similarly slighted. The impact of nonmilitary black-white relations on the military racial situation during these years is also inadequately shown. For example, the effect of contemporary race riots on white society's attitudes toward black officers is never made clear. Similarly, the New Dealers' inactivity on the black military matter is not discussed in any depth, while the book's early chapter on black experience in the nineteenth-century military is not thorough. Patton does attempt to put this book into a historical context, but his effort is not successful. A perusal of his bibliography and notes indicates the absence of some important secondary books and articles, and

this fact may help explain the book's weak historical perspective.

Still, this book is a useful addition to the growing body of literature on the Afro-American role in the United States military. Its five-page conclusion (pp. 173-77) is particularly helpful. Busy historians will appreciate the availability of this brief synopsis.

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PHILIP M. FLAMMER. *The Vivid Air: The Lafayette Escadrille*. Athens: University of Georgia Press, 1981. Pp. xi, 249. \$17.50.

On December 31, 1981, Colonel Charles H. Dolan, the last surviving member of the Lafayette Escadrille of World War I, died. His "going west" marked the final passing of the colorful American volunteers who formed a unique and elite unit that flew and fought for France before their own country entered the conflict. For the cause of France these young pilots risked not only their lives but also the loss of their citizenship during the years of official U.S. neutrality. A World War I combat pilot's life span averaged less than fifteen hours flying over the lines. Only six of the thirty-eight American members of the Lafayette Escadrille survived the war. After the unit's adventures were reported in the American press, its members became heroes at home and helped to turn American isolationist sentiment toward the Allies.

The Lafayette Escadrille, N-124, which entered the service of France on April 20, 1916, was made up of a handful of idealistic Americans commanded by French officers. In the following months they fought in the air at the battle of Verdun. They also served with distinction in all subsequent campaigns on the French and Belgian fronts. This single squadron was officially credited with downing 199 enemy airplanes.

Although the United States entered the war on April 6, 1917, it was not until February 18, 1918, that the French Lafayette Escadrille was reorganized as the American 103d Pursuit Squadron. General Benjamin Foulois, the commander of the Air Service of the American Expeditionary Forces, did not regard these American pilots already flying for France as leaders with which to build an American team of combat aviators. At the time of the transfer from French to American command this was the first and only American combat squadron at the front. For many months only French-made aircraft and weapons were available for use by the American Air Service.

Philip M. Flammer has written a clear, fully documented study based on extensive research in Europe and the United States. This work contains

an excellent account of the development of the aerial arm as it evolved from missions of reconnaissance, bombardment, interdiction, close ground support, and pursuit aviation. *The Vivid Air* is an interesting story of this remarkable group of American volunteer flyers in World War I and of their place in military aviation. Flammer also surveys the postwar history of the Lafayette Escadrille as depicted and often distorted in the popular press and films. This book is enhanced by the inclusion of a number of useful maps and photographs.

In 1920, the French Armée de l'Air officially designated the newly created 7th Squadron of the 35th Aviation Regiment as the heir to the name and tradition of the original Lafayette Escadrille. Just prior to World War II the N-124 tradition was transferred to the 1st Escadrille of the 2/5 Groupe de Chasse. This unit flew in combat in World War II, in Indochina, and in the Algerian conflict. It still exists as an elite French unit flying with the famous Sioux Indian head insignia adopted by the American flyers in 1916.

SAM HAGER FRANK
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MICHAEL T. ISENBERG. *War on Film: The American Cinema and World War I, 1914-1941*. Rutherford, N.J.: Fairleigh Dickinson University Press or Associated University Presses, London. 1981. Pp. 273. \$27.50.

Michael T. Isenberg's "study in the use of evidence" (p. 10) asserts that historians have overlooked film as a source, and a discussion of some films reveals "commonly held American ideas, attitudes, and values connected with various facets of World War I" (p. 10). Fourteen "somewhat disconnected essays" (p. 10) touch on a methodology for film historians, the possible value of film to historians, why documentary films are not always faithful depictions of actuality, plus brief thematic assessments of allies, enemies, women, humor, the home front, pacifism, and the need for commitment in various feature and documentary films. 1914-41. The author admits that some notable films about World War I have been made since 1941. He concludes by suggesting that "the fresh insights that war films of the period provide us may help to reshape some of our ideas concerning American attitudes toward the conflict" (p. 215).

Isenberg has published in recent years several parts of his book in essay form and expanded his analysis of *The Big Parade* (1925) into a notable contribution to O'Connor and Jackson's *American History/American Film* (1979). The strident call in *War on Film* for historians to begin using film in writing history, given the considerable number who have,

makes the author sound like an intellectual Rip Van Winkle. There is no reference to recent important books about World War I and World War I films, including those by Lawrence Suid, Kevin Brownlow, Paul Fussell, and Stephen Vaughn.

Contrary to what Isenberg says, there are valuable manuscript materials for some of the films he describes, the *Congressional Record* is not by definition a reliable guide, *Triumph of the Will* was released in 1935, the historian can attempt to estimate audience response to films, the federal government first made use of film in 1906, and D. W. Griffith's *Hearts of the World* (1918) amounts to much more than Lillian Gish's memoirs would lead one to believe. Russell Merritt has located rich manuscript materials in England about Griffith's official arrangements with both the British and French governments. Merritt's findings will surely encourage historians to reconsider the importance of feature film to official war aims.

Deficient and outdated in research, incoherent in methodology, chaotic in organization, wrongheaded in judgment, *War on Film* should never have appeared in print.

DAVID CULBERT
Louisiana State University,
Baton Rouge

ELLIS W. HAWLEY, editor. *Herbert Hoover as Secretary of Commerce: Studies in New Era Thought and Practice*. (Herbert Hoover Centennial Seminars, number 2; Papers Presented at the Herbert Hoover Centennial Seminars, Herbert Hoover Presidential Library Association, West Branch, Iowa, 1974.) Iowa City: University of Iowa Press. 1981. Pp. xii, 263. \$19.95.

This volume, the second of four volumes based on the Herbert Hoover Centennial Seminars, is a worthy follow-up to the initial volume, *Herbert Hoover: The Great War and its Aftermath 1914-23*, edited by Lawrence E. Gelfand. It will be followed by volumes on Herbert Hoover as president and Herbert Hoover in the postpresidency period.

The volume is edited with an introductory historiographical essay on Hoover by Ellis W. Hawley. It also includes essays by Robert K. Murray on Hoover and the Harding cabinet; Hawley on Hoover and economic stabilization, 1921-22; Robert H. Zieger on Hoover and labor; Joan Hoff Wilson on Hoover's agricultural policies; Melvyn P. Leffler on Hoover and American foreign policy; Joseph Brandes on Hoover and antimonopoly; and George W. Carey on Hoover's concept of individualism.

All of the essays show the increasingly significant place that Hoover is attaining in American history. Hawley's introductory essay is particularly helpful in setting the stage in that he analyzes the changing

historiography and shows the major topics that currently dominate writings about the 1920s and Hoover. The opening of both the Warren G. Harding and Hoover papers in the 1960s accelerated interest in the 1920s and Hoover, and these essays, while not intending to provide a comprehensive history of Hoover's activities as secretary of commerce, focus on important selected aspects of that story.

Murray's essay gives strong evidence of the importance that Hoover had in the Harding cabinet and in fact how insistent Harding was that Hoover join his administration. Murray also analyzes Hoover's managerial impulses as well as his bureaucratic expansionism, as more and more came under the authority of the Department of Commerce. According to Murray, Hoover was a modernizer and a rationalizer, rather than a defender of traditional institutions. Hawley's essay on economic stabilization shows that as early as 1921 and 1922 the federal government was attempting to create tools to manage a peacetime economy, with Hoover taking the lead. Zieger's essay stresses the management-engineering vision that Hoover brought to labor relations and Hoover's unsuccessful attempts to bring about increased productivity. Joan Hoff Wilson's essay analyzes the relationship between Hoover and Secretary of Agriculture Henry C. Wallace and also Hoover's reactions to the McNary-Haugen bills. Hoover's successful efforts to make the Department of Commerce more active in international affairs and his unsuccessful efforts to turn America away from the isolationism that it was moving toward in the 1920s are covered in detail by Leffler. Brandes's essay treats Hoover's reactions to the British rubber cartel that threatened the expansion of America's automobile industry in the 1920s. Here again Hoover stressed the idea of business collectivism and industrial harmony. The final essay by George Carey analyzes the political and social thought of Hoover and concludes that it was neither moderate nor liberal but rather neotraditional.

In all of these essays, Hoover emerges as a thoughtful, if at times unrealistic, modernizer of the American political system. This volume is certainly a worthy addition to literature on the 1920s.

EUGENE P. TRANI
University of Missouri,
Kansas City

JOHN DOUGLAS FORBES. *J. P. Morgan, Jr.: 1867-1943*. (Publications of the Graduate School of Business Administration of the University of Virginia.) Charlottesville: University Press of Virginia, 1981. Pp. xiv, 262. \$20.00.

Although John Pierpont Morgan, Jr., survived his more famous father by thirty years, the son never

emerged in historical literature as an influential banker-philanthropist with an identity separate from the older Morgan. Yet it was Morgan *junior* who played a major role in financing and supplying the British and French governments in World War I before and during American intervention, and it was his firm that was mostly responsible for underwriting and managing postwar loans to assist Western Europe's reconstruction of its shattered economies.

John Douglas Forbes purports to have written the first "full-length life" of the younger Morgan. *Caveat emptor*, for this is not a biographical study in the conventional sense; it is really a reconstructed, year-by-year diary of Morgan's comings and goings, eatings and meetings, and givings and spendings. There is no historical narrative nor even an attempt to discuss at length selected events or interests as they touched Morgan's life. A scholar interested in fresh material on the Morgan banking firm and its senior partner's investments and syndicate participations during the period will have to look elsewhere. There is scant information about Morgan's financial career, and almost all of it comes from contemporary periodicals and secondary works. Readers searching for an account of Morgan's newsworthy 1934 appearance before the Senate banking and currency committee (whose chief counsel was Ferdinand Pecora) would do better to consult the recent history of American investment banking by Carosso, Sears, and Katz.

The strong point of this book lies, then, not in its depiction of how Morgan worked but rather of how he lived. During his adult life, he spent one-third of every year as a veritable English country squire, hunting grouse, attending horticultural fairs, and dining with nobility. He donated much money to educational institutions, hospitals, museums (especially the Metropolitan Museum of Art and the Museum of Natural History in New York), and the Episcopal church, and he continued the magnificent book collection that his father had begun and that is now housed in the Pierpont Morgan Library. He was an avid, tasteful reader and, when time permitted, attended the theater and the opera.

Morgan was contemptuous of politicians. He was not remorseful at Theodore Roosevelt's death in 1919 ("At last the Lord and I see eye to eye"), and he considered Warren G. Harding a "jelly-fish." His attitudes toward Catholics and Jews were more ambivalent. He privately derided Ambassador Joseph Kennedy as an "Irish Papist," but he accepted the Grand Cross of St. Gregory the Great from Pope Pius XI in 1937 for having offered the Vatican astute investment advice over the preceding ten years. Morgan betrayed a genteel antisemitism when criticizing Samuel Untermyer and Louis D. Brandeis for their attacks on the "money trust," but

he simultaneously (and anonymously) supported needy Jewish medical students at Harvard and was a close friend of Jacob H. Schiff.

In sum, Morgan was complex and not uninteresting. A serious, full-length biographical study of the man still lies ahead.

IRVING KATZ
Indiana University,
Bloomington

RONALD STEEL. *Walter Lippmann and the American Century*. Reprint. New York: Vintage Books. 1981. Pp. xvii, 669. \$7.95.

This full, highly detailed, and well-written biography contains rich information on and insight into its subject, his writings, and his ideas, work, and personality. Peripherally, it provides glimpses into journalism, radicalism, the *New Republic*, the Inquiry, World War I propaganda, the interwar years, political and economic leaders in the United States and Europe, Jews and Zionism, an array of interesting friends of Lippmann, and a host of historical events. Ronald Steel is properly critical of his subject where he feels criticism is merited, and happily he evaluates Lippmann in the context of his time rather than imposing 1980 standards on ideas and positions.

Steel has written a life-and-times type biography, one in which he provides concise historical background for readers, which enables him to present Lippmann's views on events. These capsules are generally reliable, but they rarely indicate that considerable differences in interpretation exist regarding the topics summarized. Lippmann's position on issues in the light of these more elaborate analyses might have yielded richer insights. This is especially true regarding Cold War revisionists and the last two hundred pages that focus on relations with the Soviets and United States policies of containment and intervention.

While this is an exceptional biography, it confines itself to analyzing Lippmann piecemeal. There is no overall assessment of the man or his work. This is unfortunate, because after reading Steel's perceptive insights one is puzzled how Lippmann could have enjoyed the prestige he did. He wrote well, possessed intuitive insight into many issues and events, knew and at times even influenced the "greats," raised important questions, and aroused widespread commentary on his columns and books. Yet Steel's portrait is that of an essentially shallow man despite his brilliance. Lippmann shifted significantly in his views on business and bankers, the League of Nations, the Depression, the New Deal, presidential candidates, economic theory, civil liberties, neutrality, isolationism, his feelings as a Jew, and the role of the United States in the world prior

to 1941. Steel often describes him as confused and uncertain even as he wrote with authority. Lippmann had no ability to perceive lasting trends and never developed inner convictions. Only in his certainty that there were limits to United States power after 1945 was Lippmann consistent for any long period of time.

Two other features are troublesome. Nowhere does the author elaborate on the subtitle, a phrase that has already produced one dissertation; hence, its meaning remains obscure. Second, the word liberal, often associated with Lippmann, is not adequately examined in either its historical context or in relation to Lippmann. This is important, because Steel shows that Lippmann could be strikingly illiberal on topics one usually associates with liberals, such as the internment of Japanese-Americans and governmental loyalty programs. Despite such points, Steel must be commended for his tour de force. He digested Lippmann's massive writings and correspondence, organized this information into an intelligently conceived sequence of chapters, and provided a fascinating narrative. He has done what many persons thought would be an impossible task.

WARREN F. KUEHL
University of Akron

CAROL MORRIS PETILLO. *Douglas MacArthur: The Philippine Years*. Bloomington: Indiana University Press. 1981. Pp. xviii, 301. \$17.50.

Carol Morris Petillo's study offers something more than the title promises. It goes well beyond a simple analysis of MacArthur's Philippine years to an adventure in psychohistory, using the developmental theories of Erik Erikson as its principal base. Douglas MacArthur, Petillo contends, developed certain tensions and anxieties in his early life, brought about by the death of his infant brother and the "enormous expectations" his mother and father imposed on him. These tensions and anxieties produced an extremely close relationship with his mother, an intense desire to emulate his father's heroism in war, and avoidance of marriage and probably of sexual relations until he was in his forties. Following the Erikson formula, Petillo postulates that MacArthur developed "defenses and strategies" to meet his tensions. Of those that worked best for him, one of the most important was "undoubtedly a periodic retreat to the island haven which from the outset had provided him with a structure which supported his psychic needs" (p. 247).

Thus in 1903-04 he found opportunities in the islands for the heroism and professional development that fulfilled his romantic ideas of military life; during two tours in the twenties he compensated for disappointments in Washington by his success in the

Philippines; and in 1935 after an unrewarding tour as chief of staff he avoided relegation to military obscurity by becoming military adviser to the Philippine government. His presence in the islands as war approached led to his subsequent appointments as commander of American and Allied forces in the area. After defeat and escape in 1942, as supreme Allied commander in the Southwest Pacific, he fulfilled his burning desire to return to the Philippines in triumph, the capstone of his military career. But in resolving political difficulties after the reconquest, he was not so successful, and "gladly" left the islands in 1945 to perform on a broader stage.

There are flaws in this scheme of things. The psychohistory is highly speculative, as the very frequent use of "probably" and "may have" testifies. Nowhere is this uncertainty more pronounced than in the speculation about MacArthur's sexual inadequacy. The pattern of retreat to the Philippines after failures elsewhere also seems tenuous. For instance, MacArthur's greatest successes in the early years came in World War I, not in the Philippines. The choice of Luzon rather than Formosa as the target of the main Allied attack in 1944 derived more from the success of Admiral Halsey's raid on Leyte in September than from any of MacArthur's obsessions. MacArthur's disappointments in the Philippines in 1945 were hardly the impelling factor for his departure to take the lead role in the occupation of Japan.

All in all, this is an interesting, provocative, and controversial study, but hardly convincing on all counts. Petillo's story of MacArthur's acceptance of \$500,000 from the Philippine government in 1942 as a reward for his services as military adviser is not, however, in the realm of controversy. She proves without doubt that he did so, and the controversy is whether he acted with propriety and in accord with army regulations. Petillo denies that her study was designed to "denigrate the greatness that rightfully adheres to Douglas MacArthur," saying that she only intended to dispel some of the myths. Yet the volume will certainly dismay his admirers, quite enrage his adulators, and rather delight those who have always felt this heroic figure had clay feet.

ROBERT W. COAKLEY
Alexandria, Virginia

LILLIAN ASHCRAFT WEBB. *About My Father's Business: The Life of Elder Michaux*. (Contributions in Afro-American and African Studies, number 61.) Westport, Conn.: Greenwood Press, 1981. Pp. xix, 210. \$25.00.

The black pentecostal Church of God in Christ showered a few brightly burning splinters as it moved north from Mississippi in the first decades of the twentieth century. One fragment, Elder

Lightfoot Solomon Michaux, flashed for the 1930s decade as the brightest, or at least the most widely tuned-in minister in America. Twenty-five million Americans turned their Saturday night radio dials to his "Happy Am I" storefront showmanship, fundamentalist doctrine, and New Deal political message.

Michaux was born in Virginia in 1884 of African, Jewish, and native American ancestry. He worked in his father's seafood business until almost middle age before turning to religion and a revival tent to build a small chain of East Coast congregations centered in Washington, D.C. There he ran one of the most effective private welfare programs in the capital along with media events such as an annual baptizing at Griffith Stadium with a pageantry and fireworks that regularly drew interracial audiences of twenty thousand. He lived on well past his radio prime, promoting Presidents Harry S. Truman and Dwight D. Eisenhower, accumulating real estate speculations of more than twenty million dollars for his church, and peevishly siding with FBI Director J. Edgar Hoover against Martin Luther King, Jr.

Lillian Ashcraft Webb writes with authority on the man and the sect into which she was born as a third-generation member. While defending Michaux from being classified with cult contemporaries Father Divine and Daddy Grace, she is not uncritical. As a teenager, she seems to have rebelled against the aging leader's growing conservatism and the regimen of Mrs. Michaux's "Purity Club" with its exhortations to remain a "peach out of reach." She is unsympathetic to Michaux's authoritarian control, neither sharing power with his people nor permitting them to keep associations with those outside the church. Children were even separated from parents on occasion. The teenager who became pregnant was put out of the church, and then her parents were required to oust her from their own home. All who violated the stringent moral codes were driven out.

This study does more than narrate Michaux's colorful rise from peddling fish to evangelizing on national radio. Webb gives us a window to a small black sect much like the main-line black pentecostal religion, the third largest black brand of religion, of which we know so little. Because Michaux's own Church of God never grew much beyond twelve hundred members, Webb can write with some knowledge of the members as well as their leader. In chapters entitled "Ain't Got Time to Sin," "To Control a Holy People," and "A Business of Religion," she lays out what members gave and received economically and psychologically. This good book interprets Michaux as "neither saint nor sinner" but offers evidence that could support either case.

DAVID M. TUCKER
Memphis State University

JOHN B. KIRBY. *Black Americans in the Roosevelt Era: Liberalism and Race*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1980. Pp. xvii, 254. \$14.50.

This book accomplishes two tasks: it traces the ideas and activities of a small group of whites who sought to integrate the principles of racial liberalism into the broader scheme of New Deal reform; and it examines how some influential Afro-Americans responded to the political and economic developments of the 1930s, as well as the ideas and programs of the "white race liberals." Others have dealt with the general topic—Harvard Sitkoff most recently—but John B. Kirby's focus on ideas and ideology represents a unique contribution to the history of New Deal race relations.

The first part of the book analyzes the thought of those few white racial liberals instrumental in the New Deal, most notably Will Alexander, Clark Foreman, Harold Ickes, and Eleanor Roosevelt. They shared the belief that the combined effects of the Depression and the New Deal had fundamentally altered American institutions and values. The tenets of individualism were being replaced with those of a "new democracy" that guaranteed economic security for all regardless of race. Racial classifications and considerations were being transcended in the American mind by class conditions. Defining the "Negro problem" as basically economic, the New Deal liberals were convinced that general economic reforms would alter significantly the material status of black Americans, further modifying the racial prejudices of whites. Because the black future was tied so closely to the fulfillment of the Roosevelt reform program, these liberals could accept compromises at black expense, while cautioning blacks to be patient and not push for rights that would alienate whites and jeopardize the larger reform program. Thus black needs were diluted by judging them on the basis of what whites would accept, a process stemming from the realities of political power as well as the ideological proclivities of white liberals.

Influential blacks responded to these currents in a variety of ways. Mary McLeod Bethune and Robert C. Weaver worked within the New Deal and generally shared the outlook of their white cohorts. Ralph Bunche, John P. Davis, and A. Philip Randolph were critical of New Deal shortcomings and sought to enlarge black political power through coalitions with the white working class. W. E. B. Du Bois emphasized organization of black communities for self-help. In addition to the programmatic shortcomings of the white liberals and the lack of economic and political power, blacks were hampered by "the lack of unity and cohesion within the black community, and the lack of any clear ideological

perspective in which they might judge and determine the actual meaning of the many government programs which emerged after 1933" (p. 226).

The New Deal thus resulted in an ambivalent legacy for black Americans, both expanding and limiting the concept of equality of opportunity in the welfare state. Although the subtlety and rich texture of Kirby's insights are impossible to convey in a review of this length, the book is a thoughtful and perceptive work whose influence will be evident in future scholarship.

RICHARD M. DALFUME
State University of New York,
Binghamton

MARK S. FOSTER. *From Streetcar to Superhighway: American City Planners and Urban Transportation, 1900–1940*. (Technology and Urban Growth.) Philadelphia: Temple University Press. 1981. Pp. xiv, 246. \$19.95.

In many current interpretations, the automobile is blamed for much of the physical and economic deterioration of American cities in the twentieth century. By this interpretation, the automobile encouraged the middle class to leave the city altogether and also crippled mass transit. With the decline of mass transit, structured land-use patterns gave way to suburban sprawl. Mark S. Foster's study of the role of the planner in urban transportation in the first forty years of the twentieth century is based mostly on the experiences of Detroit, Boston, Seattle, and Los Angeles, and to a lesser extent of Denver, New Orleans, and Chicago. He concludes that however much the unrestrained reliance on the automobile may seem unfortunate in hindsight, planners at the time did not see it as incompatible with sound urban growth.

Central cities, he argues, were not pleasant places at the turn of the century, largely because of excessive congestion. Street railways were responsible for much of that congestion because they forced all traffic through the central business district, whether it needed to go there or not. The lumbering trolleys congested the streets, were noisy and maddeningly slow, averaging little more than four miles an hour through the downtown areas. Subway construction was enormously expensive and disrupted city life for long periods. Once built, all mass transit facilities were irretrievably fixed and inflexible. In contrast, the highway accommodated both private cars and buses for the public, and, to the extent that cars still made mass transit necessary, buses seemed cheaper and more flexible than rail operations.

In addition, the street railway companies were wretchedly unpopular. City planners were none too secure in their positions, subject to political pressures and annual budget reviews, and were there-

fore in no position to take up unpopular causes. Many planners gave up on central cities altogether as hopelessly spoiled and turned their attention to the airy green spaces of the suburbs. Early planned suburbs were compatible with mass transit, like the trolley suburb of Shaker Heights, Ohio. But in the long run, the automobile was more adaptable than the trolley to the suburbs' pattern of diffused population.

Within Foster's study are some important vignettes of the American political economy. For example, while planners might try to remain strictly professional, the planning commissions they had to deal with often represented vested interests—realtors, construction firms, and street railway operators—who had particular axes to grind. It was another case of private interests hoping to use government for their own purposes. And yet, these interests did not always grind their axes—like the street railway operator who waxed eloquently about the joys of motoring or the automobile manufacturer who noted that if the trolleys were sunk, the cities were sunk. For the most part, they seemed to be people who were neither evil nor conspiratorial, though they shared a Crolyesque vision of the compatibility of public needs and private desires that typifies much of what is called “progressivism.” Throughout, there was an almost pathetic insistence by the street railway operators that all was well, even as their house was burning down, either because they hoped to deceive investors on the extent of their collapse or because they themselves could not face up to the fact that they were failing.

Foster has written a provocative and important book that is filled with valuable insights. By focusing on planners, however, whose role was often peripheral, the book does not provide a comprehensive analysis of the interaction of political and economic forces in any one city. By focusing entirely on United States cities, the book fails to explain why cities in other parts of the world seemed to take a more restrained attitude toward the automobile. The book is appropriately illustrated, although many of the illustrations have been reprinted elsewhere. Foster's study was supported in part by a grant from the Ford Motor Company Foundation. While the author explains that the foundation did not try to influence his judgment in any way, his controversial conclusions, sustaining as they do the position of automobile manufacturers, would have been a lot stronger if he had been more judicious in his search for grants.

RICHARD SAUNDERS
Clemson University

A. MCGEEHEE HARVEY. *Science at the Bedside: Clinical Research in American Medicine, 1905–1945*. Balti-

more: Johns Hopkins University Press. 1981. Pp. xix, 554. \$17.50.

A. McGehee Harvey, retired professor of medicine at Johns Hopkins and a prolific researcher and writer on the history of medical science, has written what is billed as “the first comprehensive history of clinical research in the United States.” The author briefly traces the foreign influences, primarily German, that paved the way for the development of clinical medical research in the United States in the decades following the American Civil War. He explains how the premedical program in biology established by Yale's Sheffield Scientific School helped to establish the principle that medical study should be preceded by a grounding in the sciences, and, as expected from his earlier writings, he focuses on the important role of Johns Hopkins University. He describes the role of philanthropy in the development of the Rockefeller Institute for Medical Research, and he evaluates the important function of the Association of American Physicians and the American Society for Clinical Investigation. In addition, Harvey details the various developments in the field and in the process provides a valuable chronicle of the history of clinical research in medicine.

Unfortunately, the book is disappointing in many ways. First, as a history it is disappointing in its failure to develop adequately a framework upon which to place the various events and developments described in the book. Each section consists of a seemingly endless list of names and accomplishments, once in a while concluding with the heading “other major contributors.” There is very little attempt to tie the material together, except in the summary following each major section of the book. The summary should have been the basis for the text, rather than an afterthought. The problem of organizing such a wealth of information is demonstrated by the author's decision to include a final section of fifteen chapters, “The Pattern of Development in Selected Medical Centers.” Then, the author neglects to discuss the very important questions posed by E. Richard Brown in his widely debated book, *Rockefeller Medical Men*, as to the motives of the philanthropists and their lieutenants and how that influenced the direction of scientific research in clinical medicine.

Although the book may be disappointing as history, as a chronicle of developments it is an important start in the direction of a fuller understanding of the history of American medical science. In addition, although it does not at all question the motives of either the researchers or their supporters, it demonstrates the results of that philanthropy, which helped to improve the quality of medical care in the more developed nations. As

such, it is a worthwhile antidote to Brown's analysis. Finally, it provides an exhaustive survey of primary sources on the history of medical science, and it will lead future historians in the right direction.

MARTIN KAUFMAN
Westfield State College

DONALD S. NAPOLI. *Architects of Adjustment: The History of the Psychological Profession in the United States*. (National University Publications Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1981. Pp. 176. \$20.00.

In recent decades historical interest in the process of professionalization has intensified. There are a variety of reasons for this development, not the least of which is the heightened appreciation of the significance that organized groups—particularly those whose legitimacy is based in part upon the claim that specialized training and knowledge justifies authority and power—play in modern society. In *Architects of Adjustment*, Donald S. Napoli has attempted to provide a case study of the rise of the psychological profession in the United States from about the turn of the century through the Second World War and beyond.

The modern discipline of psychology emerged toward the end of the nineteenth century. Conceived within a university setting, psychology all but rejected its earlier roots in philosophy in favor of an affiliation with the newly emerging social sciences and a desire to specify general laws governing human behavior. By 1900, Americans were already challenging European structuralism. In its place they substituted a functional approach that mirrored the importance of evolutionary thought and pragmatic philosophy. In their eyes psychology involved the study of the adjustment of individuals to their environment, and the function of applied psychology—as distinguished from academic psychology—was to facilitate the process of adjustment.

Much of Napoli's book is devoted to the emergence of "applied" psychology and the struggle of practitioners to achieve status and recognition. By the early twentieth century there was some interest in applying psychological theories to such practical problems as mental retardation, industrial efficiency, and education. The vogue of testing strengthened the belief that an applied social science could aid individuals in the process of adjustment. As psychologists moved into nonacademic occupations, the stage was set for a conflict with their academic colleagues who dominated the American Psychological Association (APA).

Napoli's book provides a useful description of the history of inter- and intraorganizational conflicts among psychologists and between psychologists and other groups, especially psychiatrists. During the

1920s and 1930s, applied psychologists struggled for status and recognition. Their minority status within the APA led to the creation of such separate organizations as the Association of Consulting Psychologists. World War II played an important role in elevating the status of the discipline, and by the 1940s the APA had been transformed into a federation of divisions that provided clinical psychologists with a platform to launch a larger public campaign for acceptance.

Napoli's work is somewhat less satisfying insofar as the intellectual development of applied psychology is concerned. His emphasis on "adjustment" often conceals rather than illuminates a discipline characterized by complexity rather than simplicity. Nor is Napoli especially persuasive in describing the process of professionalization. The absence of any discussion of the clientele of applied psychologists also vitiates the discussion. Part of the difficulty is related to the book's brevity; the author races through topics that demand more extended discussion and analysis. But part of the difficulty also reflects a too simple analytic and conceptual framework. Moreover, some generalizations (for example, the discussion of the impact of psychoanalysis in the United States) are based on a very superficial factual foundation. Despite these shortcomings, *Architects of Adjustment* is a useful addition to the growing literature on the history of psychology even if it does not represent the last word.

GERALD N. GROB
Rutgers University

ANTHONY P. DUNBAR. *Against the Grain: Southern Radicals and Prophets, 1929–1959*. Charlottesville: University Press of Virginia. 1981. Pp. ix, 306. \$16.95.

Anthony P. Dunbar's *Against the Grain* is a study of courage and commitment against overwhelming odds. The subject is a group of white Southern clergymen who steadfastly supported the causes of organized labor and racial justice from the New Deal through the civil rights movement. Individuals discussed include Howard Kester, Claude Williams, Don West, Myles Horton, James Dombrowski, and Ward Rodgers. Among the institutions and organizations examined are Commonwealth College, Highlander Folk School, the Southern Tenant Farmers Union, the Fellowship of Southern Churchmen, and the Southern Conference for Human Welfare. Although others have previously noted their stories, Dunbar has given us the most complete portrait of those Southern dissidents who combined the seemingly antithetical elements of evangelical Protestantism and Marxism.

The most important figure was Howard Kester. A native of Martinsville, Virginia, Kester epitomized

the seminarian turned social activist who became a shock trooper in the battle against Southern orthodoxy. A Presbyterian divinity scholarship brought Kester to Vanderbilt University's School of Religion where he, like a number of the South's future radical clergymen, studied under Alva Taylor, a proponent of the Social Gospel. YMCA work soon led Kester to involvements with the Fellowship of Reconciliation, the NAACP, and the Socialist party; in the process he became a protégé of Norman Thomas and Reinhold Niebuhr. Kester's voluminous correspondence, which began in the 1920s and continued until his death in 1977, forms the core of the book. It is a rich collection, and Dunbar mines it well. Kester seems to have been in touch with almost every significant protest and educational activity that occurred in the South during this period. He played a particularly crucial role in the efforts to unionize white and black sharecroppers and was as strident in his opposition to Communists as he was outspoken in his advocacy of economic justice and racial equality. For the Howard Kesters of the South, slipping out of town one step ahead of a lynch mob was a way of life. Yet somehow they managed to survive. In so doing they not only lived to see some of their ideas gain wider acceptance but left a proud legacy to future Southern dissidents, such as Dunbar, who is himself the Southern field secretary for Amnesty International.

Despite his understandable sympathy for his subjects, Dunbar does not gloss over their quirks and warts. He makes it clear, for example, that internecine struggles, especially over the Communist issue, absorbed almost as much of their energies as did their battles with the bosses, planters, and sheriffs. Ironically, allegations made by some of these radicals against others became fodder for their common enemies. Larger principles and goals at times got subordinated to the need to maintain influence within the Southern radical movement.

If *Against the Grain* has a significant weakness, it is Dunbar's failure to place his subjects within the larger context of Southern reform and social protest. He too readily accepts the radicals' characterization of more moderate Southern reform as flabby and insincere, which does not do justice either to the constraints under which it too operated or the accomplishments it was able to make. Dunbar's contention that these Christian radicals provided a link between the Populist and civil rights movements is intriguing, but it is not supported by his evidence or analysis. Despite these shortcomings, *Against the Grain* is a useful contribution, providing students of the recent South a detailed look at a fascinating group of people.

MORTON SOSNA
Stanford Humanities Center

JAMES A. GROSS. *The Making of the National Labor Relations Board. Volume 2, The Reshaping of the National Labor Relations Board: National Labor Policy in Transition, 1937-1947*. Albany: State University of New York Press. 1981. Pp. 381. Cloth \$44.00, paper \$14.95.

CLETUS E. DANIEL. *The ACLU and the Wagner Act: An Inquiry into the Depression-Era Crisis of American Liberalism*. (Cornell Studies in Industrial and Labor Relations, number 20.) Ithaca: New York State School of Industrial and Labor Relations, Cornell University. 1980. Pp. 142. Cloth \$13.50, paper \$7.95.

The passage of the Taft-Hartley Act in 1947 marked the triumph of a decade-long assault on the National Labor Relations Board. No sooner had the Supreme Court affirmed the constitutionality of the Wagner Act in 1937 than employers, Republican politicians, Southern Democrats, and even some AFL unionists launched their attacks. NLRB decisions that benefited the new CIO industrial unions particularly fueled this opposition.

The main engine of anti-NLRB activity was the House Special Committee to Investigate the National Labor Relations Board. Created in July 1939, the Smith Committee (so named after its progenitor, Virginia Democratic Congressman Howard Smith) pilloried the board for the next eighteen months. Chief Counsel Edmund Toland accused it of favoritism toward the CIO, infiltration by Communists, and reliance on inexperienced female lawyers. Critics attacked the agency's rulings and procedures, which, they claimed, penalized employers and AFL organizations.

Although it failed to secure statutory revision of the Wagner Act, the committee did force key personnel changes. Eventually its main legislative proposals formed the core of Taft-Hartley. So well had the committee done its job that, declares James A. Gross, even before the 1947 legislation the board had been transformed "from an expert administrative agency which played the major role in formulation of labor policy into a conservative, insecure, politically sensitive agency" (p. 267). Gross devotes little sustained analysis to the actual content or circumstances surrounding passage of Taft-Hartley, believing as he does that the work of the Smith Committee was the decisive factor in the reshaping of federal labor policy.

This is a solid, useful account. It rests heavily on NLRB files, other archival records, congressional hearings, and oral interviews. Gross is informative on important issues such as the activities of Communists in the NLRB, internal conflict within the agency, the importance of personnel changes in determining the board's course, and the AFL's ill-

advised support for revision of the Wagner Act. Together with his 1974 volume on the origins of the board, Gross's book is a contribution to both labor and administrative history.

Alas, his diligent research is not matched by felicity of style or sharpness of perspective. Undigested quotations, reliance on the passive voice, and occasional grammatical errors indict both author and editor. Gross has arresting observations, but his hurried comments on, for example, the quirky notions of political economy that motivated AFL critics of the Wagner Act (p. 99), the challenge of the early Wagner Act NLRB to basic power relationships in society (p. 108), and the implications of galloping legalism in NLRB proceedings (p. 218) are buried in the forbidding prose. Gross's sympathy for an activist NLRB and for the aspirations of the CIO is clear but not obtrusive. If anything, his analysis, which is largely confined to rather narrow political and legal concerns, fails to contemplate the broader implications of the Smith Committee's success.

Cletus E. Daniel's slim volume brings us back to the formulation of New Deal labor policy. According to Daniel, in the early New Deal period Roger N. Baldwin, founder of the American Civil Liberties Union, joined many liberals in despairing of reform within the context of capitalism. He and other ACLU leaders opposed the legislation proposed by Senator Robert Wagner that created the NLRB, for they believed that permanent federal presence would defuse one of the decade's most promising phenomena, the angry militancy of the working class. Baldwin especially opposed Wagner's insistence on majority rule in representation determination, for he believed that this feature would devastate radical labor organizations. The Communist party, then promoting dual unions, also opposed the Wagner Act on these grounds. ACLU records reveal close cooperation between the ACLU and party members in organizing opposition.

The ACLU and the Wagner Act is engagingly written. It is illuminating on Baldwin and on the union and focuses useful attention on the dynamics of cooperation between certain liberals and the Communist party in the 1930s. The book's claims, however, as a major statement on liberalism's response to the New Deal are inflated, for the Wagner Act episode sharply divided the ACLU and threatened to leave Baldwin isolated from his own membership.

Taken together, these two books suggest a notable irony. Baldwin opposed the Wagner Act in good part because he believed that the bureaucratization that federal legislation entailed would blunt workers' protests. After passage of the act, however, he swung quickly around to vigorous support of the law. Yet at least one reading of the story Gross tells suggests that Baldwin might have been more pre-

scient than he knew in his earlier criticisms. Even as he moved in the late 1930s to marshal ACLU support against conservative efforts to cripple the NLRB, the Smith Committee was pushing federal labor policy down the road to the very legalism, bureaucratic inertia, and de facto hostility toward militant organization of workers that he had earlier warned against.

ROBERT H. ZIEGER
Wayne State University

JOHN RICHMAN, *The United States and the Soviet Union: The Decision to Recognize*. Raleigh, N.C.: Camberleigh and Hall. 1980. Pp. xi, 287. \$21.95.

Operating on the theory that the roots of a problem bear evidence of its growth, here is yet another volume examining American recognition of Soviet Russia. The central thesis of John Richman's book suggests that the Russian experts in the Department of State, headed by Robert F. Kelley, were thwarted by FDR in their effort to prevent recognition; but because they controlled the debt and trade questions they made sure that recognition would not lead to closer cooperation. The Riga axioms suggested by Daniel Yergin, though not mentioned by name, underlies the author's arguments throughout and is stressed in the conclusion, where he suggests that the Soviet specialists never forgot their antagonisms and had only to await FDR's death to reassert their anti-Soviet influence.

With a thesis there always goes the burden of proof; its focus here is on the determination of Kelley and his students in the East European Affairs division to make sure that any proposed debt settlement would not be acceptable and that blame for failure would rest with the USSR. The arguments for this case are copiously documented from original sources. This is, in fact, the most detailed study to date on the debt negotiations and the objectives underlying them. This is both the major strength and the primary weakness of the book. There are only four pages of bibliography and only four volumes cited that appeared after 1969 and none after 1972. None of the literature of the 1970s that has appeared in the major historical journals is cited. There are eleven references to Japan without any focus on the significant role it played in moving FDR to recognition. The security motives of the United States and Russia are given only passing reference in a volume subtitled *The Decision to Recognize*.

Overall the volume is well written, but there are some irritating errors. On page 4 the Farm Credit Administration becomes an Association; William C. Bullitt becomes Bullit on page 26, and eleven paragraphs begin Bullitt or Bullitt's in chapter 3. Jeffer-

son Caffery's first name is omitted on his first appearance on page 60 as is Robert Bingham's on page 67. Tense switching provides some confusion on pages 112 and 130. Also in chapter 10 the sources run away with the narrative: there are seven *ibids.* following note 13 and sixteen of them following note 22. The author italicizes *The* when referring to the *New York Times*, and there are some misspelled words or typographical errors, for example, "tactiffully" on page 155 and "Fdr" on page 185.

There are some omissions that will puzzle those familiar with the origins of Soviet-American relations. Neither Alexander Gumberg, who helped keep the recognition question alive from 1917 to 1933, nor the parent organization of Amtorg, despite the author's emphasis on the trade question, are mentioned. Richman does, however, trace clearly the connection between Communist activity in Cuba and the State Department's reluctance to grant recognition. On balance the volume should be included in a reading list on Soviet-American relations.

EDWARD M. BENNETT
Washington State University

JASON BERGER. *A New Deal for the World: Eleanor Roosevelt and American Foreign Policy*. (Brooklyn College Studies on Society in Change.) New York: Columbia University Press, for Brooklyn College Press. 1981. Pp. xii, 177. \$20.00.

This incisive, lively account of Eleanor Roosevelt's public career portrays her as a grand champion for human understanding and indomitable defender of American interests. In the interwar years, Roosevelt was a central figure in debates over pacifism, isolationism, and the halting rearmament program. Although eloquently defending peace, she condemned appeasement, warning Americans they must defend democracy to ensure a humanitarian world. Although she was close to the power center, her direct influence on foreign policy was limited. She was careful to keep her role within the broad parameters of President Roosevelt's policies. Nowhere was this better illustrated than in her courageous but generally vain attempts to win more visas for Jews fleeing Hitler's persecution. During the war, she praised Henry Wallace's vision for internationalizing the New Deal as the economic companion to the extension of political democracy represented by the United Nations.

President Truman expanded Roosevelt's power by appointing her a delegate to the first UN General Assembly in January 1946. She waged a two-and-a-half year battle for approval of the Universal Declaration of Human Rights, becoming a strong critic of Soviet communism. She defended the containment program, broke bitterly with Henry Wallace in

1948, and was a leader in promoting the ADA. Jason Berger shows her as a staunch opponent of McCarthyism and defender of full civil liberties. Scholars who blame anti-Stalinist liberals for paving the way for McCarthyism "err in equating all forms of anti-Communism with McCarthyism and in denying the possibility of being both opposed to Stalin and McCarthy at the same time" (p. 86).

Eleanor Roosevelt's long attachment to Israel is one of the subjects most fully explored by Berger. She admired the Jews' pioneering democracy, while she had little but contempt for Arab interests. Making full use of Roosevelt's papers, her writings, and papers of some of her leading friends, Berger argues that Eleanor Roosevelt exerted great influence over Truman's decisions respecting the Middle East. The author would have a stronger case had he conducted research in the Truman and Acheson papers and State Department files. Roosevelt's overly zealous backing of Israel was her chief shortcoming, as she generally spoke out in the Eisenhower years for broad global humanitarian understanding. Berger's volume is a contribution not so much for new knowledge about how Eleanor Roosevelt viewed the world but in bringing together the many threads of her commitment to an international order based on compassion for human rights.

THOMAS M. CAMPBELL
Florida State University

C. HARVEY GARDINER. *Pawns in a Triangle of Hate: The Peruvian Japanese and the United States*. Seattle: University of Washington Press. 1981. Pp. x, 222. \$25.00.

This is the first, and it may prove to be the most definitive, volume that has ever been written about the Peruvian-Japanese interned in the United States during World War II. Everyone knows of the one hundred and ten thousand or more Japanese-Americans, seventy thousand of whom were American citizens, who were interned during the war; but very few, then or now, ever heard of the approximately eighteen hundred persons of Japanese descent living in Peru who were arrested, deported to the United States, and confined here.

C. Harvey Gardiner discusses two aspects of this story in his interesting book: (1) the policies of the United States and Peru in handling this matter—both governments believed that some of the Peruvian-Japanese might be Axis agents, and both entertained very anti-Japanese attitudes; and (2) a most detailed account of these unfortunate people before the time of their arrest and deportation from Peru through their internment in the United States. Their ultimate fate was primarily the responsibility of the United States. Some were repatriated during

the war in exchange for Americans held by the Japanese. The rest were kept here for some time after the end of the war as red tape in Washington and negotiations between the United States and Peru delayed decisions as to their ultimate destination. Some returned to Peru; others chose to go to Japan; and still others were allowed to remain in the United States.

There is very little in print about these Peruvian-Japanese, and the author has relied almost solely on manuscript material and personal interviews in this country, Peru, and Japan. Most manuscripts were located in the National Archives, the Washington National Records Center, and the files of the late Wayne M. Collins, the American Civil Liberties Union attorney in California who offered his services not only to the Japanese-Americans but also to the Peruvian-Japanese. The materials in Washington include files from the Department of State, records of the Immigration and Naturalization Service, and a multitude of papers from other agencies including the FBI, the Department of Justice, the navy, and the army.

Gardiner, understandably, is most sympathetic with the Peruvian-Japanese. Their only crime was in their ancestry and in being in Peru during the war. The author is quite critical of Franklin D. Roosevelt and the governments of the United States and Peru. In concentrating almost entirely on the now-acknowledged injustices done these people, he has slighted the role of public opinion in the United States. The book would have reflected that historic period more completely if Gardiner had pointed out that, not only Franklin D. Roosevelt, but the American people as well were very angry at the Japanese after the Pearl Harbor attack; and for a few months after December 7, 1941, there were fears among many residents on the Pacific Coast, although later proven to have been unfounded, that Japan might attack some part of the West Coast.

EDWARD O. GUERRANT
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YEHUDA BAUER. *American Jewry and the Holocaust: The American Jewish Joint Distribution Committee, 1939-1945*. Detroit: Wayne State University Press. 1981. Pp. 522. \$25.00.

Yehuda Bauer is an insightful and productive scholar whose writings cover a broad range of topics related to the destruction of European Jewry during the Second World War. His most recent work, *American Jewry and the Holocaust*, is a valuable addition to the growing number of works dealing with various aspects of the catastrophe.

In this volume, Bauer does not deal with the full

range of American Jewry's responses to the Holocaust. He does deal with the work of the American Jewish Joint Distribution Committee (JDC), American Jewry's primary agency for overseas relief. In examining the far-flung endeavors of the "Joint," he recounts the valiant but hopelessly inadequate efforts initially to bring relief to the beleaguered Jewish communities of Europe and later to rescue surviving remnants. The work is a chronicle of the tragic fate of multitudes and the heroic rescue of small but significant numbers through a variety of escape hatches from Portugal to Shanghai.

Bauer clarifies the changes in Nazi policy during the course of the war and the growing crisis as the tide of Nazi conquest covered ever-increasing areas of the European Continent. The rapidity of Nazi expansion and the unanticipated escalation of ferocity in Nazi action made it virtually impossible for bystanders to comprehend—much less respond—to the tragedy that was unfolding before them.

In the early phases of the war, relief, which would ameliorate suffering and help sustain life, health, and morale, was still a reasonable response to the predicament of Jews under Nazi rule. The JDC office in Berlin was open until June 1941, and some emigration from Germany and Austria continued until October of that year. JDC assistance to suffering Jewish communities in Poland and other occupied countries did indeed increase the likelihood of survival. In Bauer's judgment, "by 1942, they had established a modus operandi that would probably have enabled them to survive the war had the Nazis not embarked on mass murder." Unfortunately, by that time, the Nazi policy had changed from expulsion and oppression to extermination. In June 1941, this policy was initiated in the newly conquered areas of the Soviet Union and early in 1942 instituted in Polish death camps. When word of the new policy leaked out, it was never fully assimilated—"information did not mean knowledge."

Bauer's work is particularly illuminating in dealing with attempts to save Hungarian Jewry in the spring and summer of 1944. This tragic episode, which includes the Joel Brand mission and the alleged "Jews for trucks" ransom offer, has been the focus of ongoing controversy with political as well as scholarly ramifications. Bauer's treatment is comprehensive and balanced and is a valuable corrective to many of the ill-informed and biased accounts that have been written.

The JDC was handicapped by its commitments to "legality," its focus on relief as opposed to rescue, the meagerness of its financial resources at least until 1944, and the lack of cooperation by allied and neutral governments in facilitating rescue operations. Nevertheless, even as millions were doomed to extermination, tens of thousands were helped to survive. Bauer's work meticulously documents the

details of scores of actions in every country that illustrate what was accomplished and demonstrate his conclusion: "J.D.C. acted in the way it understood best: energetically spending its funds to help a maximum number of people . . . it became, in country after country, in situation after situation, the embodiment of an embattled Jewish people."

LEON A. JICK
Brandeis University

ROY DOUGLAS. *From War to Cold War, 1942-48*. New York: St. Martin's Press. 1981. Pp. 224. \$22.50.

This is an excellent book that probably will not receive the attention it merits. A work written for scholars, it contains no sensational disclosures and is totally devoid of the chatty anecdotes that make for a "good read." It is an important contribution to the literature on the origins of the Cold War, however, and should be read by anyone interested in the subject. Well-researched and tightly argued, this book by a British scholar can best be described in American terms as "post-Orthodox." Those still enamored of revisionist interpretations will derive little comfort from its conclusions.

Roy Douglas's starting point is that Josef Stalin was implacably hostile toward, and suspicious of, all the Western powers. His attitude was shaped by a combination of Marxist dogma, Russia's historical experience, and his own nature. Stalin had no blueprint for world domination. Indeed, as Douglas points out, there is little evidence that he would willingly "sacrifice one soldier or one kopek for the cause of anybody else's revolution" (p. 180). What he did mean to do was advance Russia's national interests as he defined them by any means available. These included Popular Front tactics during one period, the Nazi-Soviet Pact during another, collaboration with Great Britain and the United States when it was profitable, and trying to drive a wedge between them when that promised even greater rewards.

Douglas draws a strong indictment against both Churchill and Roosevelt in their dealings with the Soviet Union. For different reasons, he believes, both men succumbed to the *hubris* of thinking they personally could convert Stalin to their views about the nature of the postwar world. Permitting their hopes to govern their actions, they ignored or discounted the abundant evidence that indicated that Stalin intended to achieve his objectives by any device short of war. Still, he claims, the situation was fluid as late as 1946, and greater unity and determination on the part of their successors could have prevented much of what happened subsequently. Russia did not succeed in eastern and central Europe because of its power but because Russia ex-

ploited Western irresolution. The strong response to the Berlin blockade marked the turning point, according to Douglas, for it showed the Soviet Union that the West would back off no longer. He believes that response was critical for the security of Western Europe and for halting the drift of events that "inevitably" would have led to an "all-out shooting war at a later date" (p. 194).

If Douglas presses his case too strongly, and this reviewer thinks he does, his book cannot be dismissed as just another tract by a cold warrior. Trying to compress his views in a short review makes them appear simplistic, which they definitely are not. There is much to argue with in this work, but it should be taken very seriously.

ROBERT JAMES MADDOX
Pennsylvania State University

CHARLES M. DOBBS. *The Unwanted Symbol: American Foreign Policy, the Cold War, and Korea, 1945-1950*. Kent, Ohio: Kent State University Press. 1981. Pp. xii, 239. \$18.50.

This monograph is part of a growing literature on America's relationship to Korea from 1945 to 1950. Charles M. Dobbs makes extensive use of archival materials in the United States, dissertations, and other secondary literature. His treatment of Syngman Rhee's maneuvers during 1949 for a Pacific pact and of the Truman administration's simultaneous efforts to push a Korean aid bill through Congress are the most detailed to date. Most of the book is devoted to tracing the evolution of an American commitment to Korea. Appropriately, Dobbs emphasizes a concern for American prestige over any strategic consideration. He criticizes American leaders for failing to grasp the complex nature of Asian nationalism and for attempting to maintain a foothold in Korea.

Dobbs ignores some important issues. He fails to analyze the significance of Harry S. Truman's succession to Franklin D. Roosevelt. In discussing the Potsdam conference, Dobbs says nothing about either the clear linkage between America's refusal to negotiate agreements on Korea and Britain's concern over the fate of its own and Italy's colonies or the probable linkage between that refusal and the successful test of the A-bomb. In treating the withdrawal of American troops from the peninsula between 1947 and 1949, he ignores the reversal of the army on the strategic importance of Korea. Prior to September 1947, army planners viewed the peninsula as of real significance, but then they went along with the Joint Chiefs of Staff in concluding that the United States had "little strategic interest" in maintaining troops there. Although Dobbs discusses the later trend toward increased military spending,

he ignores the limitations imposed on this movement by Truman's continuing fiscal conservatism and commitment to the welfare state, Congress's election-year tax cut, and Louis Johnson's tight-fisted reign over the Defense Department; and he devotes too little space to the State Department's apprehensions regarding the total withdrawal of U.S. soldiers from Korea. Thus the complex interplay of ideas, bureaucracies, and domestic politics never emerges.

The same may be said of Dobbs's treatment of the months preceding June 1950. Intelligence reports regarding North Korea's strength or intentions, Ambassador John J. Muccio's warnings, State Department efforts to increase military aid to Korea, and the decline of guerrilla activity there are all ignored. By February 1950, Dobbs asserts, "the executive branch [in Washington] had backed itself into a corner" on Korea, yet he offers little explanation as to why, having done so, American policy makers failed to protect the perceived commitment.

In tracing the evolution of that commitment, Dobbs emphasizes the deteriorating relationship between the United States and the Soviet Union and American rhetoric. This framework needs to be broadened to include the impact of the experience of the 1930s, which taught American policy makers that the United States must play a central role in world affairs and must prove to friend and foe alike that it had the will to sustain an activist foreign policy. It is within this context that Korea became an important "symbol" of American credibility.

A comprehensive treatment of American policy toward Korea from 1945 to 1950 remains to be written.

WILLIAM STUECK
Purdue University

GEORGE LIPSITZ. *Class and Culture in Cold War America: "A Rainbow at Midnight."* South Hadley, Mass.: J. F. Bergin. 1982. Pp. ix, 254. Cloth \$24.95, paper \$12.95.

One cannot read this study of postwar America without having mixed feelings. On the one hand, George Lipsitz does an excellent job of describing the state of working-class culture in the forties, a culture that ranged from the ballads of Ernest Tubbs to the hubcaps of 1949 Fords. On the other hand, his attempt to link that culture to postwar labor developments seems contrived at best. A rigid ideological perspective and a rather narrow research approach have flawed what could have been an intriguing study.

Lipsitz spends the first two-thirds of his book describing what he believes were the key labor developments of the forties. Some of these are

predictable: the no-strike pledge, the 1946 strikes, Taft-Hartley, and the CIO purges. Other developments seem to be included more for their ideological than for their historical importance: the existence of wartime wildcat strikes, the surprising (to the author) cases of labor solidarity in bitter local strikes, and the prevalence of working-class racial incidents. Both are detailed in a heavy-handed, ideological manner that leaves little doubt about Lipsitz's biases. Yet, to a serious historian, the overtones are less important than both the narrowness of his research base and the inaccuracies of some of his generalizations.

One would assume that an author who makes broad statements about the substance of UAW collective bargaining in 1946 would at least consult the UAW record, most of which is available at Wayne State University. Similarly, an author who pretends to be an authority on the CIO purges should make reference at least to the substantial records of the Association of Catholic Trade Unionists at Wayne State University and the Catholic University of America. Neither appears in Lipsitz's references. Similar problems appear in his coverage of Taft-Hartley and Truman.

The author also has a tendency to make gross generalizations that do not bear close scrutiny. He characterizes Taft-Hartley as being almost identical to the labor reforms demanded by Truman in 1946. Any serious comparison of the 1946 Norton bill and the 1947 Hartley bill would find many more differences than similarities. He explains wartime strikes as class antagonism and calls the number of such strikes "high." A more careful review of the statistics would find that number low and would explain it as little more than a teething problem of the brand new grievance system. He calls the famed 1945 full employment bill a ploy of monopolistic corporations. Yet, he does admit that it was opposed by both the NAM and the Chamber of Commerce. He even terms poor old William Douglas a "Wall Street lawyer," a title that would have shocked Douglas and Wall Street alike.

Only in the last three chapters does the author seem more interested in research than ideology. There, he delves into popular culture ranging from *Salt of the Earth* to Bob Wills and the Texas Playboys. The last three chapters, however, do not atone fully for the first nine. A full exploration of the linkage between Ernest Tubbs and Walter Reuther, if there is one, remains to be done.

JAMES C. FOSTER
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IRVINE H. ANDERSON. *Aramco, the United States, and Saudi Arabia: A Study of the Dynamics of Foreign Oil*

Policy, 1933–1950. Princeton: Princeton University Press. 1981. Pp. xiii, 259. \$18.50.

If any oil lubricated United States diplomacy in the Middle East, Cordell Hull reflected in his memoirs, it was indeed Saudi Arabian oil. During World War II and the immediate postwar years, U.S. government officials and oilmen alike began a search to secure and develop what promised to be one of the greatest economic prizes of the century—Arabian oil.

Irvine H. Anderson has provided us with a stimulating and well-documented study of those years—concentrating primarily on the companies' efforts to secure Saudi oil from the acquisition of the concession in 1933 to the fifty-fifty profit-sharing arrangement of 1950. Using the Aramco experience as a vehicle, he searches—albeit with mixed results—for some larger insights into the dynamics of U.S. foreign oil policy. What emerges is a study of the interaction between company officers and government officials, who were frequently more aggressive in pursuit of Saudi oil than the oilmen themselves.

Anderson divides the major actors in his story into three broad and ideal types: strategic planners, primarily those action-oriented but long-range strategists like Harold Ickes, secretary of the interior and head of the PWA, and Undersecretary of the Navy William Bullitt, who by 1943 were concerned about the adequacy of future U.S. reserves and the possibility that the enormous potential of Saudi oil might fall into British hands; the diplomats, those State Department officials like Hull and lower-level oil and regional experts who toyed with negotiating an oil agreement with Britain and at the same time tried to counter London's influence in Saudi Arabia; and the oilmen, those field personnel and corporate officers who developed the concession abroad and defended it at home.

Anderson develops this cast of characters as they pursued a variety of tactics to secure and develop Saudi crude. From the Petroleum Reserves Corporation to the Arabian pipeline, to the Anglo-American Petroleum Agreement, he traces their often competing and conflicting wartime schemes to stake out Ibn Saud's oil. During these years, government officials failed to forge a coherent national policy on Middle East oil.

Nonetheless, by 1947, despite their differences with the oilmen over how best to secure Arabian oil, the State Department and the Department of Defense had begun to develop the foundation of a loose but mutually beneficial relationship that seemed to serve both public and private interests. Washington began to see the companies as an unofficial vehicle to serve the national interest, and the oilmen looked to the government for support and assistance in dealing with the Saudis. Between

1945 and 1950, this relationship was further strengthened due to an unstable Middle East and the onset of the Cold War.

The book's greatest strength is its treatment of the corporate side of a very complex relationship. Through a series of interviews with oil company and government officials, Anderson has shed new light on the companies' motives and objectives and gained important insights into the limits and strengths of oil company lobbying. He argues, quite convincingly, that oil company lobbying was not the main reason for the government's interest in Saudi oil. Moreover, on issues like Palestine, the oilmen were well aware of the limitations and risks of heavy-handed lobbying.

Despite its contributions, Anderson's book has some problems. His treatment of the government side is often superficial and there is little sense of how oil policy was actually made. Moreover, his categorization of strategic planners versus diplomats is too simple. There were many in the State Department's Office of Near Eastern Affairs and at higher levels who shared the concerns of Ickes and the navy. Finally, although Anderson's judgments on the nature of company-government relations are sound, the reader is frequently left without a real sense of how this dynamic worked. The methodological note at the end of the text does little to change this impression.

AARON DAVID MILLER
Department of State

CARL W. CONDIT. *The Port of New York*. Volume 2, *A History of the Rail and Terminal System from the Grand Central Electrification to the Present*. Chicago: University of Chicago Press. 1981. Pp. xii, 399. \$35.00.

Carl W. Condit in the second volume of his survey of the Port of New York traces the development of vital transport services from the construction of Grand Central Station to the decline of the many railroads that once ensured the city's prominence. This book follows the structure of his earlier one on the port that covered from 1815 to the erection of the Pennsylvania's mighty edifice.

Condit's books are always difficult to do justice to in a few words; he has the intriguing and maddening ability to digress on everything from turn-of-the-century motor windings to the aesthetic appeal of Romanesque architecture—intriguing because he is bold enough to attempt a synthesis of transportation, technological, business, social, and architectural histories and maddening because in a book this length, covering such a vast topic, he cannot do more than suggest lines of consideration.

The author is at his very best on technical matters; his discussion of the evolution of compatible electri-

cal systems on the various roads entering the city and on the development of the various electric locomotive types is simply superb. It is the way technological history should be written. His comments on the monuments that appeared over and about the mid-Manhattan hub, while shorter and less detailed, are much more subjective. Condit is unabashed in his dislike for the post-World War II "glass box" architecture, particularly the Pennsylvania Plaza Building that was built upon the ruins of McKim, Mead, and White's original masterpiece. Less satisfying are his forays into the internal decision-making processes that led railroad executives to invest hundreds of millions of dollars in the city at the turn of the century. Condit partially rectifies this weakness by discussing the general business and economic conditions that set limits on the executives' possible actions. Also the political and social barriers that precluded an integration of transport facilities on the New Jersey side with those in New York City are only sketchily covered. He does recite most of the attempts to establish multistate boards and commissions and discusses their various proposals, but never adequately explains the reasons for their failures.

The last chapter of the book is more in the form of an extended editorial; Condit chronicles what happened to the railroads in the Northeast and the effect of the various mergers and abandonments on the port. He makes an extended and emotional plea for a nationwide policy of returning to the rails as the most energy-efficient mode of transport, citing the environmental destruction, economic waste, and overseas dependence on fuel characteristic of the present alternative means of transport. It is an argument straight out of the Association of American Railroads' press releases and moreover, one that requires immense federal aid. Nevertheless, the defects of the book are those of attempting more rather than less, and Condit must be congratulated for accomplishing as much as he did in so small a space.

JAMES A. WARD
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STEPHEN SALSBUURY. *No Way to Run a Railroad: The Untold Story of the Penn Central Crisis*. New York: McGraw-Hill. 1982. Pp. xvii, 363. \$19.95.

In *No Way To Run A Railroad*, Stephen Salsbury, professor of economic history and dean of the faculty of economics at the University of Sydney in Australia, provides a fresh look at the collapse of the Penn Central Corporation in 1970. In reviewing the short life of the Penn Central, Salsbury has written a business biography—the story of David Bevan and his role in a record-breaking bankruptcy. Bevan

served as the chief financial officer of both the Pennsylvania Railroad and Penn Central from 1951 to 1970. In a penetrating foreword, Thomas C. Cochran views David Bevan as filling the role of Cassandra—one with a gift of prophecy but fated never to be believed.

When Bevan became vice-president in charge of finance for the Pennsylvania he was joining a century-old railroad known as the "Standard Railroad of the World" and one with an unbroken record of annual dividends. But by 1951 the Pennsy was in decline, suffering from a high operating ratio, a loss of traffic to highway trucks, and passenger deficits made larger by its suburban and commuter traffic. Like most American railroads the Pennsylvania was directed by a rather stodgy unimaginative management whose options were often restrained by governmental regulation. During his first dozen years on the job Bevan modernized the management by introducing a new accounting system, by pushing a systematic debt reduction program, and by helping establish a centralized computer center. He also greatly improved the pension fund and sponsored an expanded and profitable program of diversification. Bevan's missionary work ended in 1964 when Stuart Saunders moved from the Norfolk and Western to become the chief executive officer of the Pennsylvania. Saunders generally ignored Bevan's financial advice and made capital expenditures larger than his railroad could afford. Saunders was relying on economies that he hoped would result from a merger with the New York Central.

The Pennsylvania and the New York Central were merged in 1968. Saunders was the chief executive officer of the Penn Central, Alfred Perlman, formerly the top man of the New York Central, became president and chief operating officer, and Bevan the chief financial officer. Since neither Saunders nor Perlman welcomed the conservative advice offered by Bevan he was soon stripped of his control over accounting, auditing, and data processing. The absence of premerger planning and basic differences in operations and management structures of the two lines soon led to a breakdown of Penn Central operations and a resulting decline in traffic. Questionable capital expenditures, huge operating losses, and difficulties in obtaining credit led to a total collapse in mid-1970. A surprised public and federal government looked for a scapegoat, and Saunders indicated that Bevan might be their man. Several government inquiries, the most insistent being that headed by chairman Wright Patman of the House banking committee, spent years investigating Bevan, who was eventually exonerated.

Some readers may feel that the author is carrying a torch for David Bevan. If so, it is a well-documented torch. While no bibliography is included, the chapter references reveal the author's use of

many corporate documents, memoranda, trial transcripts, and committee minutes. This detailed and closely reasoned volume should find many eager readers among railroad historians, railway executives, and leaders of the business community.

JOHN F. STOVER
Purdue University

CARL ABBOTT. *The New Urban America: Growth and Politics in Sunbelt Cities*. Chapel Hill: University of North Carolina Press. 1981. Pp. viii, 317. Cloth \$19.95, paper \$9.95.

Carl Abbott argues in *The New Urban America* that the "Sunbelt" has been around since World War II. He uses census returns and population ratios to show the extent to which urban progress in American metropolitan areas between 1940 and 1970 has taken place in the South and West. This is not exactly a new revelation. Abbott's definition of what comprises the "Sunbelt" is provocative, however, to say the least. He includes all the South Atlantic states from Delaware through Florida. Then he moves west, taking in the great half-rim of states from Texas all the way around to Washington. For good measure, he includes Nevada, Utah, Oklahoma, and Colorado. Residents of Alabama and Louisiana will be surprised to learn that they do not live in the "Sunbelt"; so will people in Oregon and Washington when they find out they do.

Abbott observes that the term "Sunbelt" has gone in the 1970s from new coinage to cliché. He shows how it has become a means of describing everything from parts of the country with conservative voting habits to changing sectional boundaries. Abbott adds a new dimension to the "Sunbelt" debate by providing a definition based on urban population statistics.

He concludes, after studying the politics of Denver, Norfolk, Portland, Atlanta, and San Antonio, that by the 1970s the booster, business-oriented, city governments in the "Sunbelt" encountered the same kind of problems, notably suburban fragmentation and central city decay, experienced by older communities at an earlier date. "The postwar coalition of sunbelt cities failed in large part because of their arrogance," he states. "Central city leaders of the 1940s and 1950s generated their own oppositions by assuming the right to make decisions for their own constituents and for the larger metropolis" (p. 256). His observations are strengthened by interviews with many "Sunbelt" leaders, plus a great deal of other primary and secondary research.

Abbott's main contribution is not in showing that "Sunbelt" towns are becoming more like those elsewhere. What he does do is to demonstrate how many pundits and political experts have been wrong

about "Sunbelt" politics. It is not enough to write off the South and West as areas of reaction. With the exception of Gerald Ford, every president since John Kennedy has been from Abbott's "Sunbelt." He calls Kirkpatrick Sale's 1975 book, *Power Shift*, "flashy and small-spirited" (p. 3). Abbott's own book gives validity to the title. The new urban America has failed to act along the lines of the assumption that all cities have a community of interest.

His book adds to the "Sunbelt" debate and to a better understanding of the shifting basis of American politics. What seems to be emerging is a coalition of the South and the West against the Northeast, with the Midwest as a swing region.

LAWRENCE H. LARSEN
University of Missouri,
Kansas City

GILBERT C. FITE. *American Farmers: The New Minority*. (Minorities in Modern America.) Bloomington: Indiana University Press. 1981. Pp. ix, 265. \$19.50.

American Farmers: The New Minority is the latest in a long series of outstanding contributions to American agricultural history by Gilbert C. Fite. In this volume, Fite explores the position of farmers as a shrinking economic and political minority group in twentieth-century America. Those interested in rural society or in farmers as a social group will find little of value here, because Fite concentrates on his areas of expertise, farm politics and agricultural economics.

Fite's explanation of how farmers became a small and numerically declining economic group is strong and convincing. He shows how the economic forces operating in modern agriculture—increasing productivity due to mechanization, scientific advances, and technological changes and a chronically low farm income and standard of living—have resulted in a dwindling farm population and a concentration of production and income in fewer hands. Fite believes, probably correctly, that the number of commercial farmers will continue to shrink due to the exigencies of the cost-price squeeze, which prevents farmers from controlling either the prices they receive or the costs of what they buy, and to the economic benefits derived from larger-scale operations.

Fite's treatment of agricultural politics is weaker than his economic analysis. He believes farmers were a powerful interest group between 1920 and 1945 because of superior organization, national and international crises, and a favorable image of rural America held by urban people. In the years since 1945 the power of farmers has waned, Fite argues, because of their disunity, their decreasing numbers, reapportionment of legislative and congressional

districts, urbanization, and the declining pull of the agrarian myth on the hearts of nonfarmers. Fite thinks that the power of farmers will continue to decline, and that the federal government will become more consumer-oriented and less producer-oriented. This conclusion seems logical enough, but it may not be justified by the facts. Numbers are but one component of the power of interest groups, and often a relatively minor one. Farm groups have usually enjoyed greater successes than their numbers alone have justified, and they have often increased their power by allying with other interests outside agriculture. It is not clear that this situation will change, nor is it certain, in the present political climate, that consumers will be elevated over producers. Perhaps Fite's conclusions about agricultural politics derive in part from his concentration on the most productive—and usually conservative—farmers and their spokesmen. Despite his laudable, and generally successful, attempt to be fair and even-handed, Fite occasionally shows a hint of the whining about farmers' declining power and the cranky antilabor and anticonsumer attitudes so prominent among comfortable commercial producers.

The few doubts I have about *American Farmers: The New Minority* do not overcome my general admiration for it. It is a gracefully written and intelligent treatment of an important subject. The general reader will appreciate Fite's deft explanations of agricultural economics and politics, and the historian will benefit from his clear overview.

DAVID B. DANBOM
North Dakota State University

DAVID J. GARROW. *The FBI and Martin Luther King, Jr.: From "Solo" to Memphis*. New York: W. W. Norton. 1981. Pp. 320. \$15.95.

David J. Garrow supersedes previous journalistic revelations of FBI excesses by identifying the internal logic of the bureau's sometimes vicious efforts to combat radicalism and restrain dissent in America. Recognizing the possibility that he might himself become enmeshed in that logic, Garrow wisely warns us against assuming that to uncover previously secret records inevitably brings us closer to historical reality. He approvingly quotes Goethe's admonition that "the most important things are not always to be found in the files" before proceeding with his painstaking search for the valuable insights contained in FBI documents obtained under the Freedom of Information Act.

Although the historical significance of Garrow's findings is necessarily limited by the fact that his book is based almost entirely on FBI reports and interviews with bureau officials, he provides a crucial foundation for a general reinterpretation, using a broad variety of sources, of the life of Martin

Luther King, Jr., and of the role of the Communist party in American politics during the 1950s and 1960s. Not only does Garrow provide new evidence that the Communist party received financial support from the Soviet Union after World War II but his conclusion that two party officials (Jack and Morris Childs—code named "Solo") involved in the Moscow connection were FBI informers also prompts further questions about the significance of the internal Communist threat that played such a large role in domestic Cold War politics. The fact that FBI informants identified Stanley Levison as a financial supporter of the Communist party during the early 1950s reveals little about the motives that led Levison later to become a friend and adviser to Martin Luther King; yet Garrow's treatment of the King-Levison tie leads the reader to a better understanding of anticommunist dogmatism and the resulting guilt by association that provided a rationale for the FBI's repeated invasions of the privacy of many Americans.

Garrow argues that the FBI's justification for its surveillance and harassment of King changed as the focus of its attention shifted in the winter of 1963–64 from a concern about Communist influence in the civil rights movement to a vindictive desire to expose King's extramarital sexual activities and finally, during 1965, to an interest in King's increasing militancy and vigorous antiwar statements. He disputes the view that J. Edgar Hoover's personal idiosyncracies or the FBI's conservative ideological orientation account for the bureau's unremitting hostility toward King; instead, he asserts that the consistent theme in the FBI's persecution of King was the "paranoid style" that Richard Hofstadter saw as a persistent theme in American political history. The FBI's image of King, Garrow suggests, was shaped by attitudes its leaders shared with many other Americans: a belief that they were seriously threatened by outsiders unlike themselves who were conspiring to bring about radical change.

Garrow's book may allow the FBI to damage King's reputation in death after failing during his life, but the years of surveillance produce some images of King that differ from the FBI's consistently negative portrait. If bureau documents undermine King's postmortem idolization by revealing his human frailties, they also provide a view of a man of singular tenacity who refused to abandon friends or political positions despite his awareness of FBI prying and despite considerable pressure from high government officials.

CLAYBORNE CARSON
Stanford University

FAWN M. BRODIE. *Richard Nixon: The Shaping of His Character*. New York: W. W. Norton. 1981. Pp. 574. \$18.95.

Psychohistorians assume the great importance of irrational forces and seek to define their influence on particular people and specific events. Richard Nixon is an inviting subject for scholars with such a bent, for it seems so obvious that such forces destroyed his presidency. Before her death in 1981, Fawn M. Brodie, a professor of history at the University of California, Los Angeles, had become one of the leading practitioners of psychohistory. "That feelings about wives, children, mistresses, and friends spill over into politics and war . . . has never been seriously questioned by historians, although they may flee from in-depth exploration of the matter," she chided us in the last of her six books. "Since Nixon's own staff members have talked . . . about his 'deep dark rage,' and . . . about his paranoia, depravity, and self-destructiveness, the modern biographer has an obligation to seek out the sources of the rage and paranoia if he can" (p. 465).

Brodie's book does not focus on the presidency. It focuses instead on Nixon's life before 1964, but the book's mission is to illuminate his White House years. Only by exploring the growth of the irrational in him, Brodie assumed, could one understand his failures. "In the last years of his life when we see Nixon making choices that were essentially self-destructive," she wrote, "the Nixon character emerges as the decisive force in his life" (p. 501).

Several themes that are not commonplace in more conventional historiography receive emphasis here. One is Nixon's lying, which is presented as a major feature of his life. "He had no emotional investment in the truth," Brodie concluded (p. 504). Other themes include the impact of death, which opened up opportunities for him. Two of Nixon's brothers died when he was young, and other deaths, especially the assassination of John Kennedy, also influenced his life significantly. Survival was "the most consistent, the most remarkable, of all aspects of Nixon's life" (p. 29).

Brodie also found in Nixon a delight in punishment. His role in the Hiss case, a crucial event in his career to which she devoted two chapters, was one illustration of this. The case "put Nixon securely on the ladder to the top in politics, and . . . hardened his conviction that destructive attack was the certain way to victory" (p. 232).

And Brodie saw in Nixon a failure to love. His marriage, which she portrayed as deeply unhappy, testified to this theme. Her account of that marriage is one of the most depressing features of a generally depressing book.

As a psychohistorian must, Brodie looked closely at Nixon's early years. Making at least as much as possible out of scattered testimony, she discussed the damage done to Nixon by his "punishing" father and "saintly" mother. The father shouted, belittled, scapegoated his sons for his own failure, demanded instant obedience, and proclaimed the message that

fear, not love, rules the world, while the mother "could not communicate a sense of being much loved" (p. 502).

The book does not ignore the presidency. The author referred to it frequently and devoted the last chapter to a personal assessment of it, a predominantly negative assessment. But that chapter is essentially a summing up of what she had learned about Nixon's character, and she backed away from a detailed study of the presidency, believing that such a study, to have lasting value, must wait on the availability of the tapes.

Brodie had confidence in the value for her purposes of the materials that were available to her. She drew heavily on interviews but relied even more on published writings, especially the books of others about and by Nixon, regarding the writings of Nixon himself as especially helpful. And she brought to this material a lively imagination. She seldom let an action or a statement pass without speculating about what lay behind it.

The result is material for a biography, not a biography itself. A biography tries systematically to explain and describe the development and estimate the impact of a person. This book seeks only to define Nixon's character and is essentially a series of essays, usually short, arranged roughly in chronological order. Each one jumps about in time. They supply valuable insights, largely the products of a sensitive, probing mind, and are capable of being brought together with other materials by a scholar with wider interests and access to the Nixon papers and tapes and other presently closed sources.

RICHARD S. KIRKENDALL
Iowa State University

THOMAS R. HENSLEY *et al.* *The Kent State Incident: Impact of Judicial Process on Public Attitudes.* (Contributions in Political Science, number 56.) Westport, Conn.: Greenwood Press, 1981. Pp. xiv, 281. \$27.50.

On May 4, 1970, at Kent State University, a group of Ohio State National Guardsmen, harassed by an unruly mob made up largely of students, suddenly fired their pistols and rifles at the crowd, killing four and wounding six. It is this famous "incident" and three subsequent legal events (the 1974 federal grand jury investigation, the 1975 federal civil trial, and the 1978-79 retrial of the 1975 civil case) that are the background for this political science study. What the authors, Thomas R. Hensley, with a colleague, James J. Best, and three graduate students at Kent State, wanted to know was what effect legal decisions had on attitudes.

The authors hoped that, by studying the attitudes of Kent State University graduate and undergraduate students both before and after the federal grand

jury investigation and the two trials, they could find the answers to three questions. They wanted to know if the students (and the students, generalized, became all people) changed their minds to concur with judicial decisions; if, when a court decided opposite to a student's belief, the student's opinion of the particular court would decline (or, contrarily, if they agreed with the decision, if their opinion of it would go up); and lastly, if, whether they agreed with the decision or not, they would continue to believe in the American legal system. Their conclusions were, perhaps not surprisingly, not surprising. The answers were, in order, "no," "yes," "yes."

The book is not, however, just an explanation of the methods the authors used or a dry, statistical summary of the results of their analyses of the questionnaires. There is an excellent chapter by James Best detailing the events of "The Tragic Weekend of May 1 to 4, 1970." The volume also includes a good description of the most important of the numerous investigations and trials that dealt with the events of May 4. Both these chapters, as well as the introduction, contain useful lists of the sizable body of literature that the shooting and subsequent trials spawned.

If this generally clear, well-written, and interesting study has a major weakness (beyond its limitation to Kent State students), it lies in the area where the authors go beyond the specific conclusions they have drawn from the responses to their questionnaires. In the most striking instance, they conclude that William Muir, who used cognitive consistency theory for his *Prayer in the Public School: Law and Attitude Change* (1967), was wrong when he decided that, "judiciously used," law could change our attitudes and personalities. Instead, they conclude that in the short run courts have only limited influence on people's attitudes and that they "risk losing support in controversial cases" (p. 210). As a result, the authors believe courts should exercise self-restraint. But they do not explain what they mean by this term, nor do they even try to reply to Robert McCloskey's challenging question in *The American Supreme Court* (1960), "What are the boundaries of modesty on the one hand and 'activism' on the other?" (p. 228).

BROOKS MATHER KELLEY
Yale University

CANADA

CHRISTOPHER ARMSTRONG. *The Politics of Federalism: Ontario's Relations with the Federal Government, 1867-1942*. (Ontario Historical Studies.) Buffalo, N.Y.: University of Toronto Press. 1981. Pp. xiv, 279. \$25.00.

This volume is one of a series of studies sponsored by a board of trustees for the government of

Ontario. The approach is provincial and emphasizes the persistent tendency of the province to claim autonomy in areas of its jurisdiction, thus conflicting with the federal power that asserts its rights to greater centralization of administrative functions and its privilege to disallow provincial decisions in any field when they are found to affect the general interest of the Dominion. Such an approach to Canadian federalism does not quite accord, however, with a certain Anglo-Canadian historiography that would stress Northern nation building as opposed to Southern republicanism. For in the all-Canadian historical approach much is ignored or rejected that would not reflect unanimity of judgment on major issues confronting the nation or the Dominion. Now, in this volume, cases are studied that tell more of dissent than agreement. It is gratifying to note that Christopher Armstrong has opened new vistas, and it is to be hoped that he will inspire colleagues in other provinces to work along similar lines of approach.

The "Ontario Historical Studies" series has had a good beginning with Peter N. Oliver's *G. Howard Ferguson* (1977), Joseph Schul's *Ontario since 1867* (1978), J. N. S. Careless's *Pre-Confederation Premiers* (1980), and a *Bibliography of Ontario History* (1980) by Olga Bishop, Barbara Unwin, and Clara Miller. Christopher Armstrong's *Politics of Federalism* comes first in a row of projected studies on specific themes. It is divided into ten chapters that cover five or six major aspects of the theme.

In this brief account the reviewer will only try to elicit the two broad perspectives that mold the narrative: one is political, the other economic. Politically, from 1867 to 1942, Ontario had four prime ministers, of whom two stand out in the so-called fight against federal controls that supposedly tended to invade certain areas reserved to the provincial jurisdiction. These were Mowat (1872-96) and Hepburn (1934-42). Mowat was coupled with Macdonald and Hepburn with King, their federal homologues. These four personages set the tone to the federal-provincial disputes. In Quebec tradition has it that Oliver Mowat matched Honoré Mercier, then premier of Quebec, and similarly, in this century, Mitchell Hepburn would match Maurice Duplessis. This is slightly true. Mercier's motivation was primarily financial, whereas Mowat conducted the contest with the federal government in the manner of a jurist, through recourse to judiciary authorities against a prime minister whose conception of the Canadian Confederation had been nearest to that of a unitary state and whose tendencies were to deny the *compact theory* of Confederation. A few decisions made by the federal government without consulting the provinces were therefore regarded as breaches of contract, to which the Macdonald and Laurier governments would oppose the federal power of disallowance, a prerogative embodied in the Consti-

tution of 1867. Conflicts arose in the form of boundary disputes, that is, legal controversies relating to business corporations and the exercise of their property rights.

Economically, two major areas of dispute emerge: natural resources and social security. Much is involved in the development of northern Ontario, the Precambrian Shield, under the Mowat, Whitney, and even Ferguson regimes, as this type of development implied several dimensions, including that of transportation. The province then was engaged in a new form of resource-oriented development that related it to provincial prerogatives, according to the British North America Act; also, this form of development was bound up with international constraints that brought it under federal jurisdiction. Under the Hepburn regime, problems arising mostly from the economic depression ended up in the maze of a royal commission on Dominion-provincial relations. Insistence on the needs of the provinces for social security measures during the depression of the 1930s raised the problem of a new allocation of fiscal powers.

Hepburn took a firm stand on this issue and refused to participate in the elaboration of the federal scheme but, in the end, assented to a transfer of fiscal powers to the federal government on the ground of wartime patriotism! The decisions seemed to have been high-handedly conducted by a federal bureaucracy.

Throughout the narrative big business is omnipresent and looms large in Ontario's politics. This may well explain why Ontario seems more practical than ideological in its dealings with Ottawa and more readily disposed to side with Ottawa whenever business interests demand it. Business lobbies, the author suggests, have no interests to defend but their own. Perhaps too much in the workings of Canadian governments is imputed to federalism that should be blamed on capitalism. Or perhaps federalism is difficult under any economic system, by any means.

The "Ontario Historical Studies" series has chosen a good theme to begin with; the book is abundantly documented and the subject is not lacking in timeliness; indeed, the constitutional issue has invaded the press and other Canadian media in recent times. The book is presented in an alluring format and bears the print of a maple leaf on its front jacket, a presumption that Canada, despite current dissensions, has not ceased to exist.

ALBERT FAUCHER
Laval University

DAVID GAGAN. *Hopeful Travellers: Families, Land, and Social Change in Mid-Victorian Peel County, Canada West*. (Ontario Historical Studies.) Buffalo, N.Y.: University of Toronto Press. 1981. Pp. xxi, 197. \$20.00.

Although the historical study of Canada's social structure and demography on the basis of the routinely generated evidence of ordinary lives has come far in the past decade, David Gagan's is the first published book-length study to use such documents to examine a rural community in the still overwhelmingly rural Canada of 1851–71. Using census manuscripts, land records, probate records, and a variety of other data on the approximately thirteen thousand families (p. 9; p. 95 says ten thousand) encountered in the three censuses for the period, Gagan investigates what happened in a pioneer rural society as farmers faced the "crisis" of establishing their numerous progeny on increasingly scarce land. Notable among the responses he finds are reduced household and family sizes, increased rates of school attendance, and changes in the patterns of intergenerational transfers of land. Always mindful of the wider worlds in which rural (and by the 1860s urban) Peel County needs to be seen, he presents his material and methods effectively and fairly.

In light of his evident knowledge and immense work, however, it is disappointing that Gagan's book is not more authoritative. A few criticisms illustrate the types of avoidable analytic and interpretative problems that the book raises. Exactly when did the crisis begin, for example? Gagan's data show that net immigration to Peel was over by 1851, indeed that Peel's highest rate of population growth was before 1842, yet he invariably dates the most rapid expansion from 1842 and includes the 1850s in the period of Peel's "very intensive population growth" (p. 17). Puzzlingly, he uses the same term, "underdeveloped," to describe pioneer Peel (p. 145), the developed Peel of the 1860s (p. 146), and the highly developed agriculture of mid-nineteenth-century England (p. 97). If he is right that "between 1860 and 1870 surplus farm production in central Canada declined by 100%" (p. 14), one wonders what farmers were selling and how the burgeoning Canadian urban centers were being fed. It should be noted that in table 51 (p. 131), 37 percent should be 47 percent, 31 percent should be 39 percent, and 36 percent should be 34 percent.

More troubling is Gagan's tendency to leap beyond his evidence. For example, he assumes that correlations reveal causal relationships. Thus, he finds that of all his families, only about one thousand persisted from 1851 to 1871, and these tended to be more successful, in terms of a variety of measures, than the population of Peel as a whole. He concludes from this that those "hopeful travellers" who moved on (about whose later lives he of course knows almost nothing) were wrong, that "social improvement . . . was a consequence of persistence in this place," that "perseverance was not merely its own reward. It was the key to success on this new frontier . . ." (pp. 122, 141, and see also

pp. 96, 137–38, 148). This conclusion is neither inherently self-evident nor logically justified.

Finally, the absence of pertinent assessment data need not have prevented Gagan from spelling out much more fully the economic logic of his argument about increasing land scarcity. Since land always had a cost, as chapter 2, on the land market in Peel before 1851 abundantly demonstrates, the land crisis needs to be related to the incomes that the land generated; the crisis was, if anything, one of cost relative to family income and wealth. That is, Peel's overall man-land ratios alone portray, especially in a highly migratory society, only an aspect, and probably not the most important one, of the economic world that Gagan's *individual* families encountered.

These criticisms notwithstanding, this is a book that will prove stimulating and valuable to specialists in the field. But the casual or unwary reader, perhaps impressed by the apparent weight of so much quantitative evidence, may well come away rather more certain than he should be of how much this book represents the "real, as opposed to hypothetical" (p. 42) processes of social change in rural Canada West.

DOUGLAS MCCALLA
Trent University

D. J. HALL. *Clifford Sifton*. Volume 1, *The Young Napoleon: 1861–1900*. Vancouver, Canada: University of British Columbia Press. 1981. Pp. xii, 361. \$27.50.

Clifford Sifton was a considerable figure. From 1891–96 he dominated the government of Manitoba; never before or since has that province been able to exercise greater influence in Canadian federal politics. When he entered Wilfrid Laurier's government in 1896 he became the boss of the Liberal party in western Canada as well as a key member of a major Canadian administration. Both positions were his until he left the regime in 1905. It is clear that Sifton occupies a major place in Canadian political development.

In the first of a two-volume life, D. J. Hall provides a careful analysis of that career from Sifton's youth to 1900. We are taken through the Brandon years, the temperance agitations, and the schools question. We go with Sifton to Ottawa and observe him functioning as minister of the interior. Hall analyzes the key episodes and themes of the initial period of Sifton's federal phase: departmental reorganization, initiatives in immigration policy, Yukon administration, the purchase of the *Free Press*, and endemic factionalism within the Manitoba wing of the party. Volume 1 ends with the general elections of 1900, which constituted a personal triumph for Sifton as western leader.

Hall presents us with a most unattractive Sifton. Here is a calculating, ruthless, ambitious, deceitful,

and thoroughly nasty man. His considerable power derived from his forcefulness, persuasiveness, debating skills, abilities as a political organizer, and substantial (if largely unexplained) wealth. He seems to have been devoid of any but the most general and contextual of ideas. Like his hapless father, he was a temperance man; like many Anglophones born and educated in Ontario, he was racist in assumption and profoundly suspicious of Francophones and Roman Catholics. Despite his personal and regional constituency of independent primary producers, he saw Canada through the eyes of a metropolitan businessman. Sifton made his initial reputation as Manitoba's spokesman and strategist on the schools question, yet the policy was conceived by others. He is famous as the minister who peopled the prairies, but his basic immigration policies were inherited from his predecessors. Sifton's reputation as a tough-minded realist and brilliant administrator is belied by extensive bungling in Yukon administration and touching naiveté in his dealings with American officialdom. Demagoguery and deceit were used routinely. During the schools debate Sifton systematically fomented anti-Catholicism and Francophobia. For several years he lied about the fact that he owned the *Free Press*: he was even able to deceive his own editor on the ownership question. His approach to news management has affinities with that of later twentieth-century totalitarians.

In 1896 Sifton entered the federal cabinet as the first prairie politician of national stature. He was a major regional leader. By 1900 he was a protectionist and an ally of the railroads and business elites of Winnipeg and central Canada. What is amazing is that western revolt was so long in coming.

In this meticulously researched and well-balanced study, D. J. Hall has given us the real Clifford Sifton. The result, though excessively long, is a major contribution to Canadian political history.

DONALD SWAINSON
Queen's University
Kingston, Canada

CLAUDE BISSELL. *The Young Vincent Massey*. Buffalo, N.Y.: University of Toronto Press. 1981. Pp. xii, 270. \$22.50.

Vincent Massey was an enigma to most Canadians and not much liked by many who had dealings with him. He was an elitist with strong imperial and monarchical attachments and he had an unlimited appetite for flattery. But he was also a Canadian nationalist and a major force in the development of Canadian artistic and cultural activity. He gave freely of his time and energy and even more generously of his family's money to the encouragement of theater and music in Canada, and in later life he headed a royal commission whose findings

had a positive impact on government attitudes toward the arts and letters in Canada. He also served his country in key positions at home and abroad. He was the first Canadian ambassador to Washington, the Canadian High Commissioner in London, and Canada's first Canadian-born Governor-General.

This elegantly written book is the first part of a two-volume study of this complex man. Its author, Claude Bissell, is himself a prominent literary critic and a former president of Massey's alma mater, the University of Toronto, and thus well equipped to explore Massey's deep commitment to Canadian cultural affairs. The sections dealing with the establishment and subsequent use of Hart House at the University of Toronto, the theatrical productions and musical activities of the 1920s and 1930s, and Massey's continuous support of Canadian artists are the best parts of the book. We learn much about those vitally important years in the development of Canadian artistic and cultural life. Bissell is also very good in delineating both Massey's religious doubts and his gradual disentanglement from the values and attitudes of his family.

The book is of less value on business, political, and imperial subjects. Bissell appears to have relied all too much on the relatively narrow base of the Massey papers, for there is very little reference to the plethora of manuscript and secondary materials that would have helped him flesh out and analyze both Massey's role as head of Massey-Harris and his political activities of the 1920s and 1930s. Similarly, the treatment of the American years is superficial. What is particularly mystifying is that Bissell fails to explore or explain Massey's gathering liberalism, nor does he take time to analyze Massey's ambivalent attitudes to empire and nation. Bissell does not add to, in fact he seems unaware of, recent scholarship on nationalism-imperialism and therefore adds little to the discussion even though Massey is a key figure for any historian of ideas and attitudes in twentieth-century Canada. We also get much less than we should from a writer of Bissell's sensitivity on Massey's family and personal life, and there is much more in Massey's own memoirs, *What's Past Is Prologue*, on his various travels and the impressions they made. Bissell makes no effort to provide an integrated appraisal, perhaps because he has chosen a thematic rather than a strictly narrative approach.

This is a readable and, on some themes, a valuable book, but in general it disappoints for not having been more venturesome in both scope and analysis.

JOHN KENDLE
University of Manitoba

J. L. GRANATSTEIN. *A Man of Influence: Norman A. Robertson and Canadian Statecraft, 1929-68*. Ottawa, Canada: Deneau. 1981. Pp. xv, 487. \$24.95.

Canada's foreign service in the 1940s and 1950s was celebrated for its excellence, and rightly so. Lester Pearson, Hume Wrong, Jules Léger, and many others were by any standards, Canadian or international, outstanding public servants, who gained for their small nation an eminence it had not before (and has not since) possessed. Lester Pearson and Jules Léger are the most familiar names of that era, but, among his peers, Norman Robertson was perhaps preeminent. Charles Ritchie, the splendid diarist and Robertson's colleague, wrote of him "what does not Norman understand? His mind is as capacious as his great sloping frame. He has displacement, as they say of ocean liners, displacement physical and intellectual and he is wonderful company." But for a historian who has not shared that "wonderful company," Robertson's traces are almost as difficult to find as the wake of an ocean liner that passed long ago. His legacy is richest in the memory of his friends and is exceedingly barren in the memoranda and letters that a biographer most values.

A Man of Influence is more a study of influence than of a man. J. L. Granatstein admits in his preface that this book provides relatively little detail on Robertson's private life. Robertson, Granatstein argues, "deserves to be remembered for his deeds and *they* are the focus of this book." There is no trifling with psychohistory, but this work is not at all a "life and times." In tracing these deeds and explaining their significance Granatstein makes a major contribution to our understanding of Canadian bureaucracy and foreign policy between 1930 and 1960. The research is astonishing in its range and thoroughness. He has conducted eighty-eight interviews and has consulted manuscript collections in thirty repositories. In several cases, he has discovered collections that will be of great value for other subjects and other historians. The importance of Granatstein's work, however, lies not in his research itself but in the skillful use he has made of it.

This well-written and perceptive book is essential reading for anyone interested in Canadian political and diplomatic history. The Department of External Affairs chose wisely when it asked a leading revisionist critic of Canadian foreign policy to write the biography of Robertson. The result is a book that is remarkably generous to Robertson, the department, and the governments of those times. For revisionists of a later day, *A Man of Influence* will be the definitive account.

JOHN ENGLISH
University of Waterloo

LATIN AMERICA

DAVID G. SWEET and GARY B. NASH, editors. *Struggle and Survival in Colonial America*. Berkeley and Los

Angeles: University of California Press. 1981. Pp. 398. Cloth \$28.50, paper \$8.95.

Reading this book recalls the pronounced preoccupation of about ten years ago with popular social history and unorthodox themes. The study of the the lower classes then seemed promising, overdue, and potentially revolutionary. Some authors referred to their work elegantly as the history of the inarticulate. To others it was the defiant "history from below." David G. Sweet and Gary B. Nash, the two editors of this volume of twenty-one essays by twenty authors, describe their effort as "the experiences of such extraordinary 'ordinary' people" (p. 1). The book is a curious collection of the sketchy and not-so-sketchy biographies of twenty-three individuals scattered across the geographical as well as temporal span of colonial America. Although not all the characters were American born, their significance and historical record derived from their interplay with the evolving societies and politics of the New World. Eight of the cases were drawn from Mexico, five from British North America, four from Brazil, three from Mexico, two from Barbados, and one from Argentina.

The individual life histories encompass a variety of legal, social, and economic conditions, of occupations, natal origins, and cultures as well as situations. The common thread that binds them seems to be that regardless of time, place, or circumstances they were atypical for their respective societies and classes. The indigenous peoples all demonstrated varying degrees of success at combining, adapting, and adjusting to the new conditions created by subjugation and colonialism. Martin Ocelotl was a member of the pre-Hispanic religious elite. Imprisoned by Moctezuma, he not only survived the conquest but also craftily proselytized for the Spanish Catholic church while developing an extensive private business concern, only to be exposed eventually and ruined by the Inquisition. Isabel, daughter of the last Aztec emperor, Moctezuma, died in 1550 at the age of about forty, having had five husbands from two cultures, seven children, and influential connections from Mexico City to Cáceres, Spain. Opechancanough died in 1646 at the extraordinary age of eighty years, after trying relentlessly for nearly forty years to expel the English colonists from Tidewater Virginia. Shulush Homa tried to establish an independent power base on the fluid frontier between the French along the Mississippi Valley and the English in the Carolinas, but his plans failed with his death in 1747. Jacob Young, a Dutchman, skillfully trafficked between the English and the Indians around the Chesapeake Bay in the seventeenth century. Micaela Carrillo of Mexico defied Spanish law and tradition by including her illegitimate children in her will during the eight-

teenth century. Francisca, an Indian slave from the Amazonian city of Belem do Para initiated a suit to win her freedom and nearly succeeded in 1739. These and similar cases are reconstructed in *Struggle and Survival*.

Most of the cases are interesting and in some cases—especially those of Manuel Cardozo, Solange Alberro, Francis Jennings, and Jerome Handler—are extremely well written. But the general introduction and the short introductory essays promise a significance that escapes the reader. The rationale for the four divisions is highly obscure. In one case, the nun Catarina de Monte Sinay accumulates a fortune. Her case is considered one of "survival through individual accommodation." But when the widow Micaela Angela Carrillo does the same, it is categorized as "survival through competition." The connotation of struggle and survival is not clear in many cases, and it is difficult to think that Isabel, daughter of Moctezuma; or Hernando de Valencia, the tax official; or Enrico Martinez, the hydraulic engineer, could be considered among the "ordinary" people of their day. Although each essay offers suggestions for further reading, there is no index nor other conventional scholarly apparatus.

FRANKLIN W. KNIGHT
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GAD J. HEUMAN. *Between Black and White: Race, Politics, and the Free Coloreds in Jamaica, 1792–1865*. (Contributions in Comparative Colonial Studies, number 5.) Westport, Conn.: Greenwood Press. 1981. Pp. xx. 231. \$35.00.

American scholars, used to the insidious tradition that a person with any African ancestors is lumped with the "colored" mass, have been predisposed to believe that British West Indian society too was essentially bifurcated. This view informed Philip Curtin's seminal *Two Jamaicas* (1955), and, although increasing attention has since been paid to the "unappropriated people" in the middle, there has been a continuing tendency to suggest that the burgeoning group of Jamaicans of mixed descent was bound to cleave to one or another of the polarized sections.

Mavis Campbell argued strongly in *The Dynamics of Change in a Slave Society* (1976) that the Jamaican free coloreds, in their selfish ambitious for self-aggrandizement, identified almost wholly with the dominant whites, treating slaves, blacks, government, and philanthropists only as allies of convenience. Now, five years later, Gad J. Heuman, in a far more measured and scholarly work, convincingly dismantles this simple contention, arguing, if not quite proving, something like the opposite.

After a brief general survey of the free coloreds in slave society, Heuman shows in the first of three

main parts how in slavery's last three decades the Jamaican free coloreds were changed from loyal petitioners to active supporters of abolitionism, both by the intransigence of the white plantocracy and by the inexorable swing of metropolitan policy and opinion away from the plantocratic establishment. The key episode in this phase, the author argues, was the controversy over the treatment of the colored émigrés Lecesne and Escoffery between 1823 and 1825.

The Jamaican free coloreds were granted nominal civil equality in 1830, but, if the Jamaican plantocracy thought it had thereby won them over, it was disappointed. The most vocal free coloreds, notably Edward Jordan and Robert Osborn, continued to identify with the governor and the mercantile community—both agents of the metropolis—against the planters. This trend, argues Heuman, became ever more obvious after emancipation, as an increasing number of colored and even black men gained entry to the Jamaican Assembly, reaching a peak around 1850. Although far from being an organized party, the colored and black assemblymen commonly voted against the white planter majority, being referred to colloquially as the Town, Government, or even Liberal party.

In the book's second part, Heuman provides a useful set of political biographies, and he attempts with mixed success to make a general ideological analysis of political behavior on the grounds of race and class. Far more impressive and valuable is part 3, a chronological account in seven succinct chapters, of the shifting electorate and of voting patterns in elections and in debates on significant issues in the Assembly between 1834 and 1865. Heuman's research has been painstaking, and his methods of statistical analysis and presentation should serve as models for other colonies and other colonial periods.

Heuman shows conclusively how important non-white voters became in elections and how consistently the brown and black representatives acted in the interests of their electors and themselves, at least until 1853. It is much more troublesome to explain how and why the nonwhites almost ceased to be a unified force in the critical years between 1853 and 1865. In brief, it seems that the colored and black representatives, never a majority, became damagingly divided. Jordan and Osborn themselves were taken into the governor's council and continued to support the imperial government, but many others became disenchanted by imperial policies to the extent of siding with the planter majority in opposition to free trade and retrenchment. In this respect they were becoming Jamaicans first. Yet despite this they were unable to prevent the constitutional changes that limited the franchise and abolished the Assembly itself in 1866.

Heuman admits that his book concentrates too much on the colored leaders and on formal political activity, downplaying the exponentially increasing but variegated mass of the people of mixed descent. Further, like Curtin and Campbell before him, Heuman may be too concerned with questions of racial and political polarization. If indeed, as he documents but does not stress, the "people of color" in Jamaica covered almost the entire spectrum from black to white, occupied an increasing range on the social scale, and followed an equally wide range of political actions, then future studies of Jamaican society, especially of the postemancipation period, should surely concentrate less on questions of race and color and more on those of class and Creolization.

MICHAEL CRATON
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WALTER RODNEY. *A History of the Guyanese Working People, 1881–1905*. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1981. Pp. xxv, 282. Cloth \$26.50, paper \$6.95.

On the evening of June 13, 1980, Walter Rodney was killed by a bomb handed to him in a package that was supposed to contain a walkie-talkie. His death left a vacuum in the Working People's Alliance party and denied both Latin America and Africa of an inspiring teacher and creative scholar. Fortunately, his latest manuscript had been through first revisions for Johns Hopkins's "Studies in Atlantic History and Culture." Franklin W. Knight was able to make the final revisions and provide the index.

As the title implies, the focus of *A History of the Guyanese Working People, 1881–1905* is on the Indian and African Creole agricultural laborers. The chapters on plantation labor and the small farming sector are particularly instructive. Equally successful is Rodney's discussion of the environmental and economic constraints on agriculture. The examination of the differences between the coast and hinterland economies points to the significance of nonagricultural industries such as gold mining and lumbering.

Rodney takes issue with those studies that have tended to emphasize racial conflict within Guyana, finding "racial conflict far less pronounced than might have been expected from the manner in which the two main races were thrown into economic competition" (p. 219). He concludes that, in spite of having to compete for the same jobs, indentured Indians and free Africans realized that they had much in common. The riots of 1905 illustrate both the failure of political reforms and the recognition of the common interests of Indian and African, men

and women, rural and urban laborers, and the lower and middle classes.

Although tables provide information on the occupation of all working adults, 54 to 64 percent of which were engaged in agriculture, the text provides little information about the nonagricultural population. The economic conditions of the working peoples are carefully discussed, but there is little on the importance of religion in their lives. One learns little of the workers' families, and the position of women is discussed in detail only in connection with the riots of 1905.

Although the monograph is free from polemics and distortions of information, it lacks a strong conceptual framework. Too often the chapters seem unrelated to each other, but there are a number of themes that appear. Rodney is convincing in his arguments that the workers were economic men. The planter class preferred indentured labor because unlike free labor it did not seek increased wages and benefits. Free labor was willing to work if paid adequately. Rodney is less convincing in his discussions of worker awareness of their exploitation.

The limited material in print on Guyana makes Rodney's carefully researched book particularly useful. Twenty tables deal with subjects as varied as rainfall and ethnic composition. There are sixteen illustrations and three maps, which further enhance the volume. Finally, the discussion of the agricultural sector of the economy provides useful information that will allow the comparison of Guyana with other societies.

VERA BLINN REBER
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MICHAEL L. CONNIFF. *Urban Politics in Brazil: The Rise of Populism, 1925-1945*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1981. Pp. xix, 227. \$19.95.

Urban Politics in Brazil is a small book that promises to have a large impact on scholarship in its field. In his fine study Michael L. Conniff adds a new dimension to our knowledge of the agitated reform movements of the 1920s and the revolution of 1930 in Brazil by viewing them from an urban perspective, and he breaks new ground by offering a "historical theory of populism which stresses its urban past" (p. 19). Conniff applies the term "populism" to the experiments by reformers seeking a means to give the common people a voice in government and to integrate them harmoniously into the rapidly expanding, industrializing society of Rio de Janeiro. His persuasive analysis of the rise, eclipse, and rebirth of mass politics in Brazil should find a wide audience among students of comparative polit-

ical systems and practice as well as among specialists in modern Brazilian history.

Readers primarily concerned with political theory will find the essence of Conniff's interpretation in the introduction, epilogue, and conclusion. In these three sharply focused chapters the author traces the roots of Brazilian populism to the nation's colonial heritage of largely autonomous municipal governance of a holistic, integrated society; finds its immediate origins in the protests against the nineteenth-century liberal, patrimonial system that prized individualism over community; discusses the attractions of corporatism, another colonial legacy offered as an alternative route to social harmony in the depression decade; and analyzes the continuing struggle between populism and authoritarianism (equally hallowed in Iberian and Brazilian tradition) in terms that lend coherence to the course of Brazilian politics for the past half-century and suggest parallels with politics in other developing countries.

Brazilianists will also welcome Conniff's reconstruction of local politics in Rio de Janeiro, which constitutes the bulk of the text. By focusing on the activities and ambitions of populist leaders, rather than their lofty rhetoric, he puts the earlier protest movements and the post-1930 social legislation into the context of pragmatic politics. Their often bitter competition to organize and control trade unions, to build and staff schools and hospitals, and to manage the new social-security institutes is presented as contemporaries understood it—rivalry for patronage to be distributed as favors in exchange for votes. Much of this section is devoted to the career of Mayor Pedro Ernesto Baptista (1931-36), whose charismatic style and novel multiclass, electoral alliances attracted national attention, pro and con, until the populist option was foreclosed under the authoritarian Estado Novo (1937-45). The relationship between municipal and national politics, personified here by Pedro Ernesto and dictator-president Getulio Vargas, is dealt with in a masterful synthesis (chap. 8) that reveals how and why Vargas first encouraged, then stifled mass politics in the national capital, only to adopt a populist stance himself toward the end of his long reign. Conniff's lucid and logical explanation of Vargas's political behavior in 1945, anticipating a new brand of populism in postwar Brazil, is one of the important contributions of this book.

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Davis

JOHN LYNCH. *Argentine Dictator: Juan Manuel de Rosas, 1829-1852*. New York: Clarendon Press of Oxford University Press. 1981. Pp. 414. \$65.00.

This superb book is not so much a biography of Juan Manuel de Rosas as a study of his long and ruthless regime. With literary elegance and conceptual clarity, John Lynch combines social and economic analysis with biographical and political narrative. He bases his findings on exhaustive research in Argentine and British archives, diplomatic papers, newspapers, and secondary literature. The result is a triumph of both scholarship and style.

The book will not please ardent *rosistas*—who can be found on both the right and the left of the Argentine political spectrum. These revisionists have argued that Rosas was an economic nationalist or populist benefactor or both, but in fact, as Lynch demonstrates, he was neither. Instead, he was a tyrant who rose to power with the support of the Buenos Aires cattle barons and who used power primarily to promote the interests of that class. This is not a new interpretation of Rosas—indeed, it is a traditional one—but Lynch's evidence is convincing, and the book should become the starting point for all future discussion of Rosas.

Although Rosas identified culturally with the rural poor as well as with marginal urban groups like the Buenos Aires blacks, he manipulated rather than politicized them. He sponsored no significant distribution of wealth or land toward the lower classes, and little trickled down to them. Rosas spurned public education while his regime enacted draconian vagrancy laws that condemned the unemployed and the transient to military service. Rosas's state "was the estancia writ large" (p. 124); his populism was little more than demagoguery.

Rosas's tariff law of 1835, which some scholars have interpreted as a protectionist measure to promote national industry against foreign competition,

was basically a sham device to cultivate the political support of certain artisan groups. In any case, it provided little protection and Argentine "industry" did not thrive. The British community in Buenos Aires did not object to this tariff, a point that brings up another major focus of Lynch's argument. Rosas was anything but anti-British. He cultivated British commerce and friendship, and he even offered to trade away Argentina's claim to the Falkland Islands in exchange for a debt settlement. In sum, Rosas's economic nationalism amounted to little more than his populism.

One of the most important sections of the book reminds us that all-pervasive state terror is nothing new in Argentina. Rosas, who aimed at total political control, used systematic terror, especially in 1839 and 1842. The *mazorca*, which apologists either overlook or portray as primarily an antisubversive spy organization, was in fact a paramilitary group, closely linked to the regime, which specialized in slitting the political opposition's throats.

Lynch discusses Rosas's overthrow clearly, although a map of the military campaigns of 1851–52 would have been useful. And throughout the book, one would like to have seen a more systematic discussion of Rosas's character and personality. In fact, our clearest image of Rosas emerges toward the end of his life, when Lynch discusses his twenty-five-year English exile. Similarly, Rosas's wife and daughter were key figures in the regime, but Lynch gives them insufficient attention and they remain murky in the background. These, however, are not major objections. The book is highly recommended to everyone interested in Latin American history.

CARL E. SOLBERG
University of Washington

Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

CHARLES TILLY. *As Sociology Meets History*. (Studies in Social Discontinuity.) New York: Academic Press. 1981. Pp. xiii, 237. \$25.00.

Sociology, Meet History. Computing History. Homans, Humans, and History. Useless Durkheim. War and Peasant Rebellion in Seventeenth-Century France. How (and, to Some Extent, Why) to Study British Contention. Proletarianization: Theory and Research. States, Taxes, and Proletarians. Looking Forward . . . into a Rearview Mirror.

ERIC J. HOBSBAWM, editor. *The History of Marxism*. Volume 1, *Marxism in Marx's Day*. Bloomington: Indiana University Press. 1982. Pp. xxiv, 349. \$25.00.

ERIC J. HOBSBAWM, Marx, Engels, and Pre-Marxian Socialism. DAVID MCLELLAN, The Materialistic Concept of History. PIERRE VILAR, Marx and the Concept of History. MAURICE DOBB, Marx's Critique of Political Economy. ISTVÁN MÉSZÁROS, Marx "Philosopher." NICOLA BADALONI, Marx and the Quest for Communist Liberty. LAWRENCE KRADER, Theory of Evolution, Revolution, and the State: The Critical Relation of Marx to His Contemporaries Darwin, Carlyle, Morgan, Maine, and Kovalevsky. ERIC J. HOBSBAWM, Marx, Engels, and Politics. GEORGES HAUPT, Marx and Marxism. GARETH STEDMAN JONES, Engels and the History of Marxism. ERIC J. HOBSBAWM, The Fortunes of Marx's and Engels' Writings.

L. POMPA and W. H. DRAY, editors. *Substance and Form in History: A Collection of Essays in Philosophy of History*. Edinburgh: Edinburgh University Press; distributed by Columbia University Press, New York. 1981. Pp. xii, 198. \$20.00.

ISAIAH BERLIN, Note on Alleged Relativism in Eighteenth-Century European Thought. R. F. ATKINSON, Kant's Philosophy of History. PATRICK GARDINER, History and Morality

in Hegel's Philosophy of History. LEON J. GOLDSTEIN, Dialectic and Necessity in Hegel's Philosophy of History. DENNIS O'BRIEN, *Psyche and Geist* in History. NATHAN ROTENSTREICH, Convertibility and Alienation. REX MARTIN, Collingwood's Doctrine of Absolute Presuppositions and the Possibility of Historical Knowledge. LOUIS O. MINK, Is Speculative Philosophy of History Possible? J. L. GORMAN, Precision in History. J. R. LUCAS, Historian Malgré Moi. P. H. NOWELL-SMITH, History as Patterns of Thought and Action. W. H. DRAY, Colligation under Appropriate Conceptions. LEON POMPA, Truth and Fact in History.

DAVID LOWENTHAL and MARCUS BINNEY, editors. *Our Past before Us: Why Do We Save It?* London: Temple Smith; distributed by PRG, Springfield, Va. 1981. Pp. 253. \$14.45.

MICHAEL HUNTER, The Preconditions of Preservation: A Historical Perspective. HUGH PRINCE, Revival, Restoration, Preservation: Changing Views about Antique Landscape Features. PETER J. FOWLER, Archaeology, the Public, and the Sense of the Past. BEVIS HULLIER, Why Do We Collect Antiques? MARION SHOARD, Why Landscapes Are Harder to Protect than Buildings. TAMARA K. HAREVEN and RANDOLPH LANGENBACK, Living Places, Work Places, and Historical Identity. SYLVIA SAYER, Wild Landscape: Dartmoor—The Influence and Inspiration of Its Past. KEN POWELL, Leeds: "Obsolescence" and the Destruction of the Inner City. JOHN POPHAM, The Relict Countryside: Preservation and Change in Suffolk. MATTHEW SAUNDERS, Metroland: Half-Timbering and Other Souvenirs in the Outer London Suburbs. MAX HANNA, Cathedrals at Saturation Point? ELIZABETH BEAZLEY, Popularity: Its Benefits and Risks. MARCUS BINNEY, Oppression to Obsession. DAVID LOWENTHAL, Dilemmas of Preservation.

T. M. L. WIGLEY *et al.*, editors. *Climate and History: Studies in Past Climates and Their Impact on Man*. New York: Cambridge University Press. 1981. Pp. xii, 530. \$59.95.

M. J. INGRAM, G. FARMER, and T. M. L. WIGLEY, Past Climates and Their Impact on Man: A Review. J. GRAY, The Use of Stable-Isotope Data in Climate Reconstruction. S. C. PORTER, Glaciological Evidence of Holocene Climatic Change. H. J. B. BIRKS, The Use of Pollen Analysis in the Reconstruc-

tion of Past Climates: A Review. H. C. FRITTS, G. R. LOFGREN, and G. A. GORDON, Reconstructing Seasonal to Century Time Scale Variations in Climate from Tree-Ring Evidence. R. MCGHEE, Archaeological Evidence for Climatic Change during the Last 5000 Years. M. J. INGRAM, D. J. UNDERHILL, and G. FARMER, The Use of Documentary Sources for the Study of Past Climates. C. PFISTER, An Analysis of the Little Ice Age Climate in Switzerland and Its Consequences for Agricultural Production. S. E. NICHOLSON, The Historical Climatology of Africa. WANG SHAO-WU and ZHAO ZONG-CI, Drought and Floods in China, 1470–1979. H. H. LAMB, An Approach to the Study of the Development of Climate and Its Impact in Human Affairs. H. FLOHN, Short-Term Climatic Fluctuations and Their Economic Role. M. L. PARRY, Climatic Change and the Agricultural Frontier: A Research Strategy. J. L. ANDERSON, History and Climate: Some Economic Models. A. MACKAY, Climate and Popular Unrest in Late Medieval Castile. B. D. SHAW, Climate, Environment, and History: The Case of Roman North Africa. T. H. MCGOVERN, The Economics of Extinction in Norse Greenland. D. M. G. SUTHERLAND, Weather and the Peasantry of Upper Brittany, 1780–1789. D. C. SMITH *et al.*, Climatic Stresses and Maine Agriculture, 1785–1885. D. A. MOOLEY and G. B. PANT, Droughts in India over the Last 200 Years, Their Socio-Economic Impacts and Remedial Measures for Them. M. J. BOWDEN *et al.*, The Effect of Climate Fluctuations on Human Populations: Two Hypotheses.

HENRI J. M. CLAESSEN and PETER SKALNÍK, editors. *The Study of the State*. (Studies in the Social Sciences, number 35.) New York: Mouton; distributed by Walter de Gruyter, New York. 1981. Pp. xiv, 535. DM 105.

MARC ABÉLÈS, "Sacred Kingship" and Formation of the State. LUCIEN R. BÄCK, Traditional Rwanda: Deconsecrating a Sacred Kingdom. PIERRE BONTE, Kingship and Politics: The Formation of the State among the Pastoralists of the Sahara and the Sahel. HENRI J. M. CLAESSEN, Specific Features of the African Early State. RONALD COHEN, Evolution, Fission, and the Early State. SUSAN DRUCKER-BROWN, The Structure of the Mampusi Kingdom and the Cult of *Naam*. R. A. L. H. GUNAWARDANA, Social Function and Political Power: A Case Study of State Formation in Irrigation Society. ANATOLI M. KHAZANOV, The Early State among the Eurasian Nomads. DONALD V. KURTZ assisted by MARGARET SHOWMAN, The Legitimation of Early Inchoate States. HERBERT S. LEWIS, Warfare and the Origin of the State: Another Formulation. PETER LLOYD, West African Kingdoms and the Early State: A Review of Some Recent Analyses. JEAN-CLAUDE MÜLLER, "Divine Kingship" in Chiefdoms and States: A Single Ideological Model. A. K. NARAIN, The Kushāna State: A Preliminary Study. FRANK PERLIN, The Pre-Colonial Indian State in History and Epistemology: A Reconstruction of Societal Formation in the Western Deccan from the Fifteenth to the Early Nineteenth Century. IRMGARD SELNOW, Ways of State Formation in Africa: A Demonstration of Typical Possibilities. SUDHARSHAN SENEVIRATNE, Kalinga and Andhra: The Process of Secondary State Formation in Early India. PETER SKALNÍK, Some Additional Thoughts on the Concept of the Early State. EDWARD I. STEINHART, From "Empire" to State: The Emer-

gence of the Kingdom of Bunvoro-Kitara, ca. 1350–1890. KURUMI SUGITA, Terrestrial Deities and Celestial Bureaucrats: Transformation of the State and Local Communities in the Asiatic Mode of Production in Japan. ROLANDO TAMAYO Y SALMORÁN, The State as a Problem of Jurisprudence. ROMILA THAPAR, The State as Empire. MICHAŁ TYMOWSKI, The Army and the Formation of the States of West Africa in the Nineteenth Century: The Cases of Kenedugu and Samori State. MIKHAIL A. VITKIN, Marx and Weber on the Primary State. ROBERT L. WINZELER, The Study of the Southeast Asian State. HENRI J. M. CLAESSEN and PETER SKALNÍK, *Ubi sumus?* The Study of the State Conference in Retrospect.

JEAN BETHKE ELSHTAIN, editor. *The Family in Political Thought*. Amherst: University of Massachusetts Press. 1982. Pp. viii, 354. Cloth \$22.50, paper \$10.00.

JEAN BETHKE ELSHTAIN, Toward a Theory of the Family and Politics. SUSAN MOLLER OKIN, Philosopher Queens and Private Wives: Plato on Women and the Family. JEAN BETHKE ELSHTAIN, Aristotle, the Public-Private Split, and the Case of the Suffragists. MARY LYNDON SHANLEY and PETER G. STILLMAN, Political and Marital Despotism: Montesquieu's *Persian Letters*. MARY LYNDON SHANLEY, Marriage Contract and Social Contract in Seventeenth-Century English Political Thought. TONY TANNER, Julie and "La Maison Paternelle": Another Look at Rousseau's *La Nouvelle Héloïse*. JOAN B. LANDES, Hegel's Conception of the Family. RICHARD W. KROUSE, Patriarchal Liberalism and Beyond: From John Stuart Mill to Harriet Taylor. TRACY B. STRONG, Oedipus as Hero: Family and Family Metaphors in Nietzsche. JANE HUMPHRIES, The Working-Class Family: A Marxist Perspective. JANE FLAX, The Family in Contemporary Feminist Thought: A Critical Review. THEODORE MILLS NORTON, Contemporary Critical Theory and the Family: Private World and Public Crisis. JAMES GLASS, Kafka and Laing on the Trapped Consciousness: The Family as Political Life. JEAN BETHKE ELSHTAIN, "Thank Heaven for Little Girls": The Dialectics of Development.

HARVEY J. GRAFF, editor. *Literacy and Social Development in the West: A Reader*. (Cambridge Studies in Oral and Literate Culture, number 3.) New York: Cambridge University Press. 1981. Pp. ix, 340. Cloth \$37.50, paper \$11.95.

HARVEY J. GRAFF, Introduction. MICHAEL T. CLANCHY, Literate and Illiterate: Hearing and Seeing: England, 1066–1307. EMMANUEL LEROY LADURIE, Cultural Exchanges: Early Fourteenth-Century Montaillou. ELIZABETH L. EISENSTEIN, Some Conjectures about the Impact of Printing on Western Society and Thought: A Preliminary Report. NATALIE ZEMON DAVIS, Printing and the People: Early Modern France. GERALD STRAUSS, Techniques of Indoctrination: The German Reformation. DAVID CRESSY, Levels of Illiteracy in England, 1530–1730. MARGARET SPUFFORD, First Steps in Literacy: The Reading and Writing Experiences of the Humblest Seventeenth-Century Spiritual Autobiographers. EGIL JOHANSSON, The History of Literacy in Sweden. KENNETH A. LOCKRIDGE, Literacy in Early

America, 1650–1800. ROGER S. SCHOFIELD, Dimensions of Illiteracy in England, 1750–1850. FRANÇOIS FURET and JACQUES OZOUF, Three Centuries of Cultural Cross-Fertilization: France. HARVEY J. GRAFF, Literacy, Jobs, and Industrialization: The Nineteenth Century. TONY JUDT, The Impact of the Schools, Provence, 1871–1914. JOHANN GALTUNG, Literacy, Education, and Schooling—For What? E. VERNE, Literacy and Industrialization—The Dispossession of Speech.

KEITH TRIBE, *Genealogies of Capitalism*. Atlantic Highlands, N.J.: Humanities Press. 1981. Pp. xvi, 175. \$27.50.

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TO THE EDITOR:

Harry Kenneth Rosenthal's review of my book, *Germans, Poles, and Jews* (AHR, 86 [1981]: 870), poses questions a calmer reading would have spared him from raising. He asks whether "Germanization and an 'eastern imperialism' precede[d] German racism and, indeed, a Polish nationalist movement?" Far from affirming this query, as he supposes, I argued that, before the nineteenth century, Prussian eastward expansion was untroubled by the nationalist preoccupations locked within the concept of Germanization. Until its collapse in 1806, the government in Berlin strove to Prussianize the Poles, a prenationalist goal the Poles did not reject out of hand. The Prussian government's subsequent embrace of a policy of Germanization is one of the book's central themes. German conservatives found the rise of the Polish nationalist movement increasingly incompatible with their notion of Prussianization, although German left-liberals, Social Democrats, and Catholics saw no contradiction in the organized existence of Polish-speaking Prussian citizens. A crucial point in my argument is that official adoption of an increasingly radical, *völkisch*, or ethnically defined Germanization policy sprang not from the intensity of local nationality conflict, which was before the 1870s low, nor from the pressure of rabid German nationalists on a conservative government inclined to leave the Poles in peace; instead, Bismarck and his successors trod the path of Germanization of their own volition, applauded by a shrinking constituency, with the intent of shoring up the conservative-monarchical regime against the "enemies of the Reich." The legacy of this conservative legitimization of a policy which, already before 1914, aimed at the territorial and linguistic dissolution by authoritarian means of Polish society within the German empire, figures among the greatest misfortunes of modern European history.

To Rosenthal's second question, whether Bismarck's Polish policy was "heavily influenced by foreign policy considerations," my answer, contrary to Rosenthal's assumption, was that Germanization expressed a response to domestic problems in no sense determined by military-diplomatic imperatives. In Bismarck's view, it was the Poles' pursuit of liberal-democratic home rule that set them at eternal odds with Berlin and justified his Germanization

policies. The Poles' legalism belied his disingenuous fears of a "Polish breakaway."

Rosenthal's last question proposes that German-Polish relations represented a "sideshow" in Prussian-German history in which "indifference" was a dominant German reaction to the Poles. This view betrays an astonishing blindness to the fatal consequences of the emergence in modern German history of the impulse to resist social and political democratization through expansion, military-political and ethnic, to the east. I argued that a majority of politically vocal Germans before 1914 opposed Prussian Germanization policy, not that they were indifferent to it. Most Germans were willing to leave the Poles to their own devices, but the German government, before 1918 as under Hitler, insisted, disastrously, that Germany had a historical mission in the east. If Rosenthal wishes to trivialize German-Polish relations, he will find support neither in Germany nor in Poland nor Israel.

Rosenthal casts the originality of my work in doubt, asserting that I have merely transmitted the "local" views of Polish historians. In fact, none of the scholars in Poland have been as concerned as I to elucidate Prussian Polish policies from the *internal* development of German society and politics. Rosenthal failed to perceive the high degree to which my argument is articulated with the controversial German historical literature. Investigation of the Hugenberg *Nachlass* and Austrian diplomatic correspondence yielded perspectives on German policy and politics unsuspected in Poland. Research in Polish archives enabled me to revise prevailing interpretations of the National Democratic movement. Max Kollenscher's unpublished memoirs underpinned my analysis of the emergence of Zionism among the Poznanian Jews. Moreover, the book offers the first sustained history, from early modern times to 1914, of the social and political relations among the three nationalities in the German-Polish borderlands.

By this I mean no criticism of Polish historiography, which, especially in the work of Professors Jakóbczyk, Topolski, and Trzeciakowski, whom Rosenthal singled out by name, has attained high scholarly planes. If my work harmonizes in some respects with theirs it is because I recognize the strength of their analysis, just as I also aimed to build on the strength of German and Jewish scholarship. Rosenthal is not correct in saying American scholars interested in central Europe, an area that I, unlike him, do not regard as "a remote part of the globe," tend to adopt the "local viewpoint." The dominant tendency among Americans is to reject the scholarship they encounter there, often without learning to appreciate its nuances and controversies. Among the reasons for this negative attitude, which Rosenthal certainly expresses, is an incapacity to

think seriously about historical works of Marxian inspiration.

WILLIAM W. HAGEN
*University of California,
Davis*

TO THE EDITOR:

In fairness to my associate editors and to the friends who helped me to compile *New American World*, I would like to make a few comments on Douglas R. McManis's review (*AHR*, 87 [1982]: 244–46). I appreciate the laudatory general remarks he makes about the collection and take his specific criticisms, only pointing out that an editor has continually to make judgments that may not satisfy every critic. It is true that "most of the material in this collection was previously published," but it is not true that it does not contain "a significant number of important documents that were receiving their first modern printing." I can assure him that the additions to the documentation from English sources add all the significant documents of which I am aware and that they do add appreciably to the mass of material already available. The great majority of the documents on Newfoundland (a neglected area) had remained unpublished, and many were contributed by the generosity of Selma Barkham, Gillian T. Cell, and John Roberts from their own research. Nor is there just "an occasional new translation" but a substantial number, otherwise ten translators (from five languages) would not have been needed to be named in the foreword to volume I. The translations on Spanish Florida between 1590 and 1612, in particular, the work of Roger Wright and J. R. Fisher, filled another gap in sources available in English: others are scattered throughout a long text. The comment that some items in the collection "are being reprinted for the third or fourth time" is true, but if they were documents basic to the section in question, there was no alternative unless there were documents of equal significance available that had not been used so often. If these points are taken, it need not be assumed that the editors are complacent about their selection. They would like to have printed more in areas where documents are plentiful and still more in areas where documents are very scarce, while minor errors may well be revealed by a close scrutiny. Their choice was, and is, open at any point to challenge, provided the overall purpose of the collection is appreciated. This is to cover the ground for students in libraries where many of the documents are no longer in print and to introduce scholars to a limited number of documents, mainly covering untouched areas, that have not hitherto been available. McManis is an authority on maps, and I am sure he and the editors would have liked to see large-scale reproductions on coated paper of the 147 maps included. As it is, the reproductions are in most cases as good as could be accommodated

in the space available, though a few are, indeed, slightly blurred. It should be made clear (as it is indeed well known to McManis) that on large manuscript maps names are rarely clear if reduced drastically in scale (even if they may, or may not, be legible in the originals). Such maps are used on this collection to show the gradual emergence of the shape and scale of the American continent. They may or may not have succeeded in doing so, but this is not the point that was made. Finally, and this is said without arrogance, the concluding comment seems to be somewhat off the mark: "As a documentary collection, the set is flawed and supplements rather than supercedes its precursors." What were its precursors that attempted to cover just this ground? The project was attempted only because there was nothing of this sort in existence: it might have been better to have omitted the last phrase and to have, perhaps, concentrated more specifically on the "flaws."

DAVID B. QUINN
St. Mary's College of Maryland

TO THE EDITOR:

What Robert Griffith (*AHR*, 87 [1982]: 87–122) says of Eisenhower, one may say of him: despite his lucid intelligence, he is at heart a visionary. According to Griffith, Eisenhower sought to reconcile the conflict in modern capitalism between individual selfishness and the need for order; however, his social vision, possessing a "profoundly antidemocratic character," failed to match the forces of modern America. But whose social vision, Eisenhower's or Griffith's, is antidemocratic? Focusing on political and business leaders, Griffith readily finds "corporatism" in America but overlooks popular preferences. Does he believe that they are simply the product of clever public relations? And if so, what does that say about the democratic faith in common sense?

Consider Griffith's discussion of the Interstate Highway Act of 1956. He blames that law for contributing to the decline of mass transit and several other social ills. However, well before the Eisenhower administration mass transit fell into eclipse, mainly because Americans, especially suburban home owners, preferred the privacy, flexibility, and convenience of cars. Perhaps short-sighted, that preference certainly resulted in serious problems. Nevertheless, that kind of private consumption may also have helped reconcile Americans to the increase of discipline and organization upon their lives. Some of Eisenhower's business supporters, at least, believed that harmony within democracy had to rest on a rising standard of living. Does Griffith have in mind a democratic alternative that will not pollute, waste resources, or produce other ill effects?

ALAN RAUCHER
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American Historical Association

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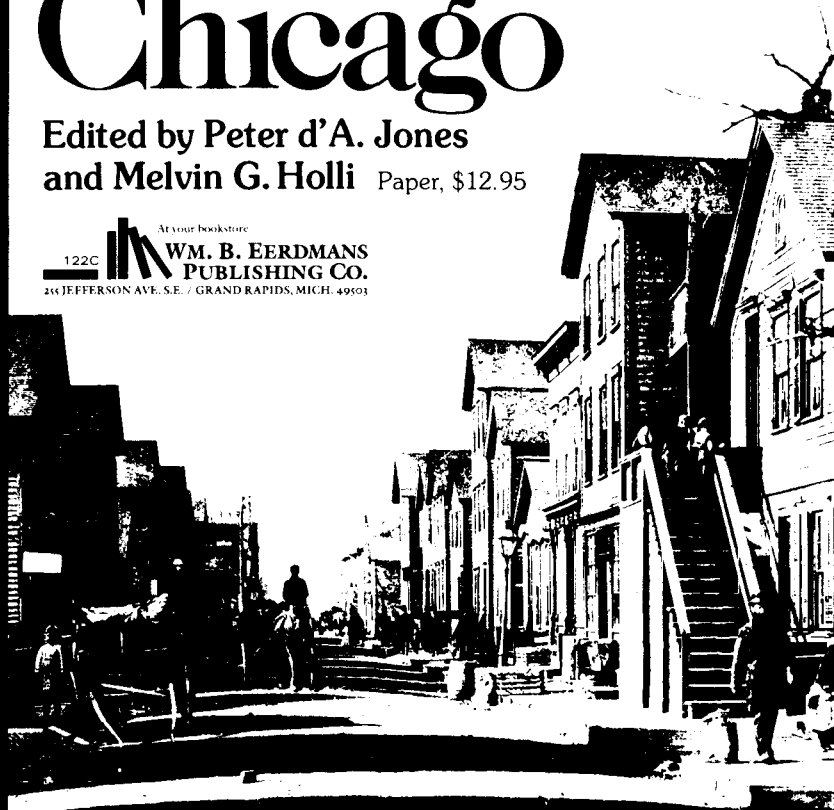


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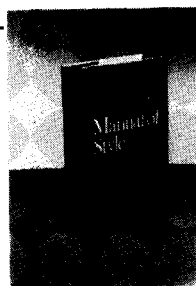
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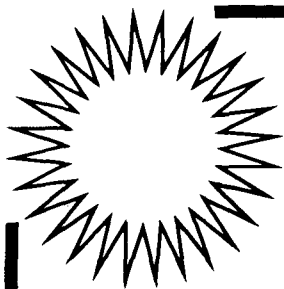
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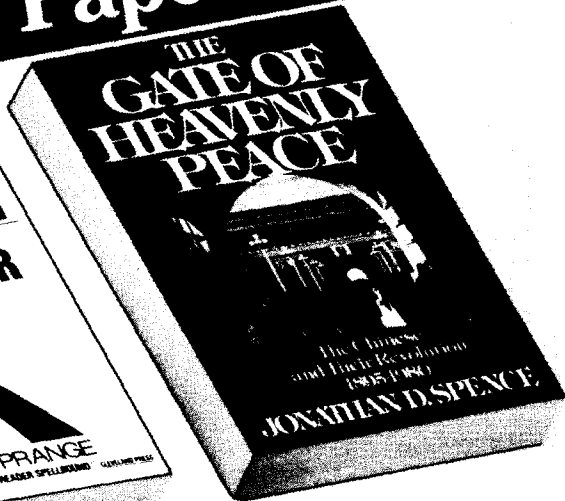
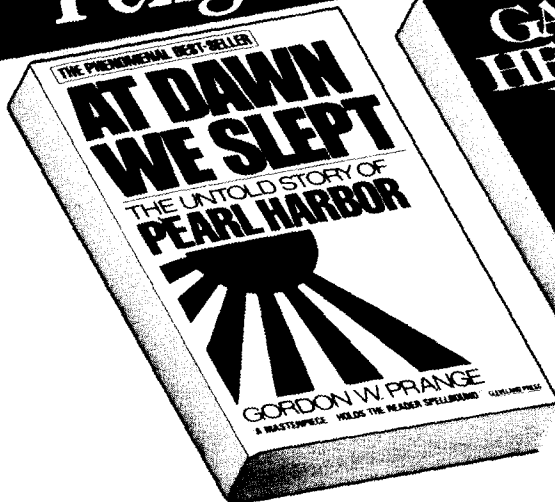
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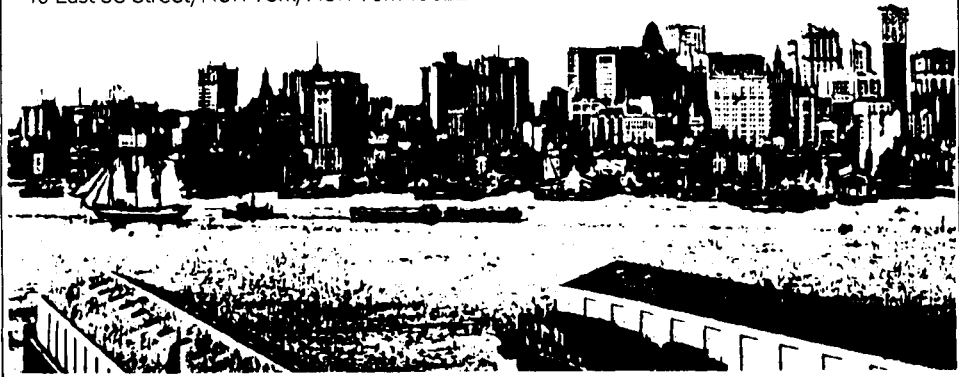
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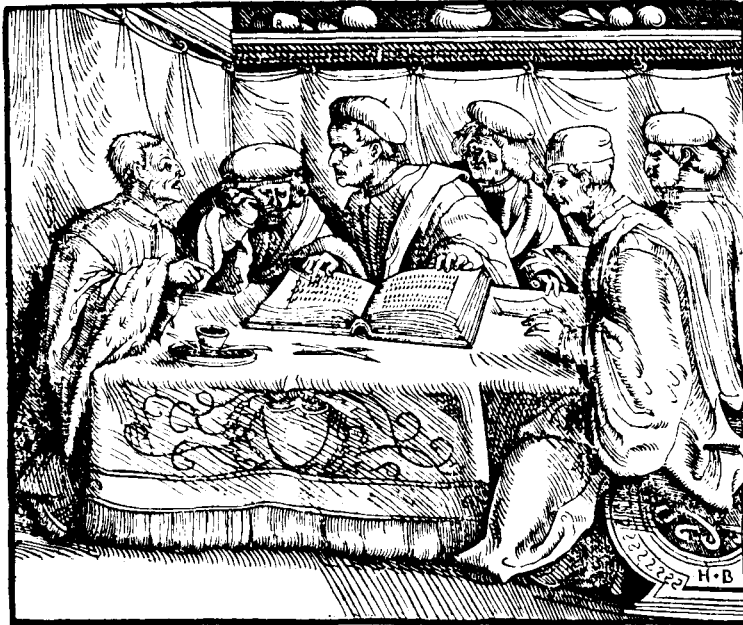
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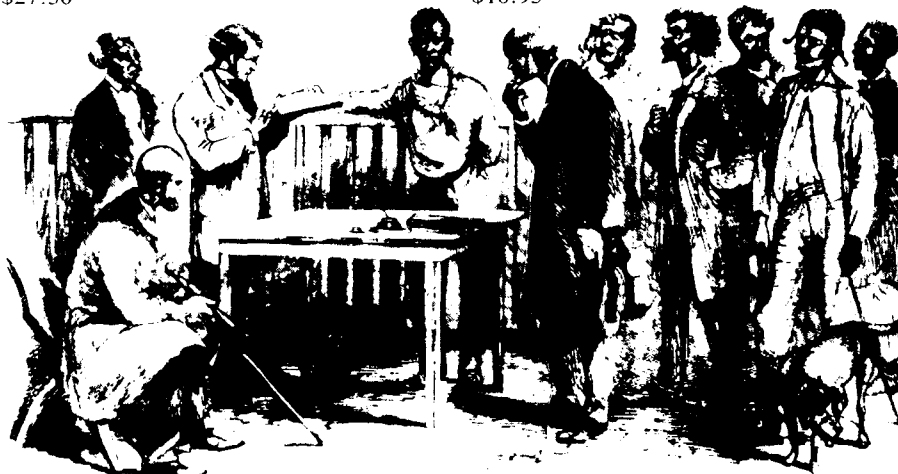
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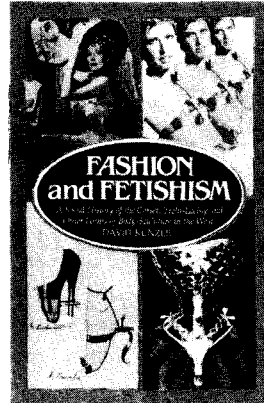
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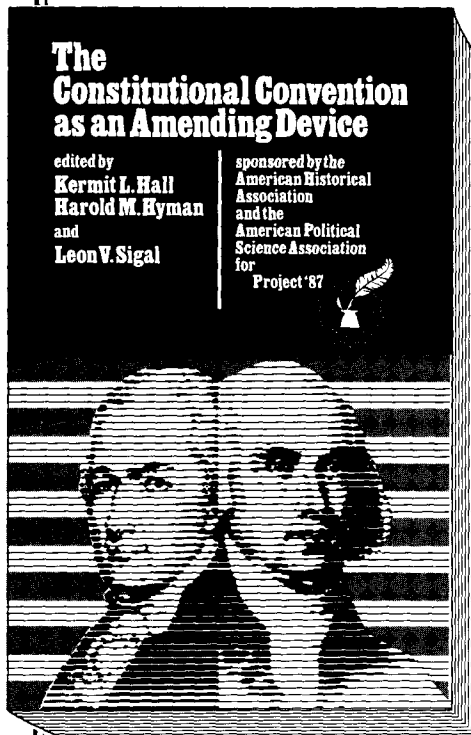
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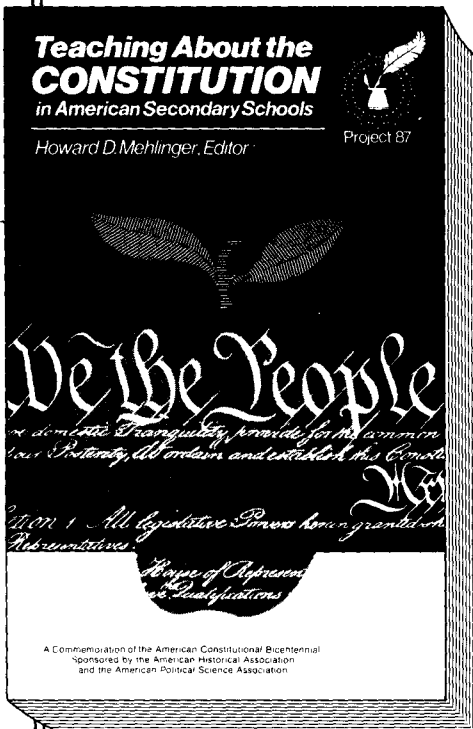
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